SCIENCE

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CONTENTS:

*SOME RECENT IDEAS ON THE EVOLUTION OF PLANTS.**

THERE is endless dissimilarity in nature. No two plants and no two animals are exactly alike. There are more plants and animals than can find a place in which to live and thrive. There results a struggle for existence. Those animals or plants which, by virtue of their individual differences or peculiarities, are best fitted to the conditions in which they are placed, survive in this struggle for existence. They are 'selected' to live. Those that survive propagate their peculiarities. By virtue of continued variation, and of continual selection along a certain line, the peculiarities may become augmented; finally the gulf of separation from the parental stem becomes great and what we call a new species has originated.

This, in epitome, is the philosophy of Darwin in respect to evolution of organic forms. It contains the well-known postulate of natural selection, the principle that we know as Darwinism. This principle has had more adherents than any other hypothesis of the process of evolution. All recent hypotheses in some way relate to it. A number of them modify it, and some cut across it. The most pronounced counter-

* Address before the Society for Plant Morphology and Physiology, Washington, December 29, 1902.

hypothesis is also the newest. It is that of Professor De Vries, botanist, of Amsterdam, who denies that natural selection is competent to produce species, or that organic ascent is the product of small differences gradually enlarging into great ones. According to De Vries's view, species-characters arise suddenly, or all at once, and they are ordinarily stable from the moment they arise.

I. VARIATION: DE VRIES.

De Vries conceives that variations, or differences, are of two general categories: (1) Variation proper, or small fluctuating, unstable differences peculiar to the individual (only partially transmitted to offspring), and (2) mutations, or differences that are usually of marked character, appear suddenly and without transition to other forms and are at once the starting-points of new races or species. The variations proper may be due to the immediate environment in which the plant lives. The mutations are due to causes yet unknown, although these causes are considered to be physiological.

Natural selection works on both variations and mutations by eliminating the forms that are least adapted to persist. It is conceived to be a destructive, not a constructive or augmentative, agency. It merely weeds out.

We may first consider selection with reference to variations proper. Among variations, or individual fluctuations, there may be a slight cumulative effect of selection, but it is incompetent ever to enlarge the differences into stable characteristics; and when natural selection ceases to act, the so-called variety falls back into its original form or splits up into other forms. Varieties of this kind are notably indefinable and unstable. It is impossible to 'fix' them in any true sense; selection only preserves them, and when it is removed they perish as varieties. They are relatively only temporary and have no effect on phylogeny. Many of the minor adaptations of plants to the particular conditions in which they chance for the time being to be placed are of this category. Much of the variation which results in acclimatization belongs here. The fluctuating horticultural varieties, and gardeners' 'strains,' are of this kind. This discussion of the effect of cessation of selection recalls Weismann's panmixia, a name proposed to designate the breaking up of varietal or specific characteristics when natural selection ceases to act. Panmixia is not of itself an original force or an agency; it is merely a name for the results of all the forces or energies which are allowed to assert themselves when the restricting force of natural selection is removed. In De Vries's view, the progress made by selection must be maintained by selection.

We may next consider selection with reference to mutations. The mutations are practically stable or 'fixed' the moment they arise. Of course there may be individual fluctuations, or variations proper, amongst plants that have sprung from a mutated individual; but the main characteristics of the mutations are heritable. An organism is a complex of organs and attributes. Each attribute is a unit. From any unit a new unit may arise by mutation. The origination of a new unit constitutes at once a full and important character and marks the organism that possesses it as a new physiological species. Not only one unit, but any number of units, may give rise to mutations; and any one of these new mutations may give rise to other mutations. But the point is that these mutations, be they great or small, arise by steps, are full-formed when they arise, and do not grow or enlarge into other mutations. The mutations are multifarious

(*all-seitig*), occurring apparently at random and in diverse directions, and without regard to fitness. They may be either quantitative or qualitative. Variations proper arise mostly in a definite line. Now, natural selection may weed out mutated individuals as it does mere variant individuals; and thus breaks may arise in the chain, and we have left what we know as taxonomic species.

Natural selection, with survival of the fittest, is, therefore, of two distinct categories,—that which operates within the species and results in the formation of local minor races, and that which operates between species and results in the formation of a line of ascent.

Everywhere and always plants are variable. Now and then and relatively rarely, plants are mutative. Any man who sees two plants, sees variation; there are no two plants alike. Only he who studies and observes critically, sees mutation. One must examine a hundred or a thousand or ten thousand individuals. In De Vries's extended experiments with *Œnothera,* only 1.5 per cent. of the plants were mutative, and mutation is undoubtedly more common in cultivation than in the wild, and the mutated individuals are more likely to persist. The investigator should employ only statistical methods of comparison. He should contrast unit-characters, rather than individuals as a whole. Moreover, not only are the numbers of mutating individuals relatively uncommon, but the species may not now be in a mutative epoch.

In other words, there are epochs in the history of the plant when mutations occur. These are the 'mutation-periods' of De Vries. There are epochs of non-mutations, when no progress seems to be making. It may be conceived that some force is then withholding or restraining the mutative impulse. This force is what we are in the habit of calling heredity. When heredity is overcome, there arises a 'premutation-period,' in which the mutations are beginning to express themselves; and eventually the full mutation-period may appear. Heredity and non-heredity, these are the ever-opposing and ever-contrasting forces of organic life, the one resulting in the survival of the like, the other resulting in the survival of the unlike. One is heredity; the other is variation. One makes for continuity; the other for evolution. No hypothesis of the energy of evolution is perfect that does not account for both. A theory of heredity, or continuity, must also account for the opposite of itself. It is not easy to construct an hypothesis or a metaphor that will accomplish this.

The phenomena of continuity and discontinuity are well contrasted by Korschinsky. These phenomena, he conceives, are the results of two antagonistic tendencies. Under normal or usual conditions heredity is the stronger force. The tendency to vary is always present, being predisposed by environment but not caused by it; when it gathers the necessary energy it overbreaks the power of inheritance and sudden variations or sports arise, and these sports are the starting-points of evolution. This sudden appearing of new forms is called by him heterogenesis.

The conceptions of *per saltum* variations of Korschinsky and De Vries seem to be practically identical. De Vries has carried his work further, into the realm of actual experimental investigation. He studied many species of plants in the hope of finding one or more that might be in its mutation-period. Finally, he chose the common evening primrose, *Œnothera Lamarckiana,* and by continual sowing of seeds and raising of great numbers of plants he discovered several truly mutative forms. These forms reproduce themselves by means of seeds as accurately as accepted species do. He has given them specific names. The full

experimental history of them is given in the first volume of his brilliant work, 'Die Mutationstheorie.' These forms, he contends, are true elementary species. That is, they have new specific characters. These characters are heritable. It does not matter whether these characters are large or small—they become phylogenetic. These plants having the new specific characters may not be species in the Linnæan or historic or morphological sense, but they are real entities. We must give up the historical view of species when we study the evolution of organic forms. Historic or Linnæan species are taxonomic conceptions; the evolutional or elementary species are physiological conceptions.

The different categories of species, as respects their origin, are given as follows by De Vries:

A. Origin by means of formation of new characters, or progressive species-origin.

B. Origin without formation of new characters.

1. By the becoming latent (*latentwerden*) of present characteristics, or retrogressive species-origin. Atavism in part belongs here.
2. By the becoming active (*activirung*) of latent characteristics, or degressive species-origin.
   (*a*) Taxonomic anomalies.
   (*b*) Real atavism.
3. By means of hybrids.

It will now be seen that the mutation theory of De Vries, which is in some respects a rephrasing and an extending of the old idea of sports, does not of itself introduce any new theory of the dynamics of evolution. It is not a theory of heredity nor of variation. His hypothesis of 'intracellular pangenesis' carries the explanation of these phenomena one step farther back, however. The plant cells give off pangenes. Each of these pangenes divides into two. Ordinarily, these two resemble the parent; but now and then one of them takes on a new character—the two become unlike—and gives rise to a mutation. This hypothesis, like Darwin's pangenesis, is useful as a graphic basis for discussion, whether or no it has real physiological foundation.

The most emphatic points of the mutation theory, as they appeal to me, are these: (1) It classifies variation into kinds that are concerned in evolution and kinds that are not; and thereby it denies that all adaptation to environment makes for the progress of the race. (2) It denies the power of natural selection to fix, to heap up or to augment differences until they become truly specific. (3) It separates the results of struggle for existence and survival of the fittest into two categories, only one of which has an effect on phylogeny. (4) It asserts that evolution takes place by steps, small or great, and not by a gradual unfolding or evolving of one form into another. (5) It enforces the importance of critical comparative study of great numbers of individual plants or animals. (6) It challenges the validity of the customary conception of species as competent to elucidate the method of evolution.

There will arise confusion, in the forthcoming discussions of the theory of discontinuity, as to what is a species; but this confusion is not new. There are two conceptions of species: (1) As taxonomic groups, more or less arbitrarily made for purposes of classification; (2) as real things, marked by recordable differences, however small or great, and conceived to be the actual steps in the phylogeny of the race. These categories are so distinct that they would not be confounded except for the unfortunate circumstance that we use one word (species) for the two. There has been a growing conviction that the two

classes must be sharply separated when evolution questions are discussed. Nearly ten years ago I endeavored to combat the species-dogma from the garden point of view, as, in differing ways, others had done before ('Survival of the Unlike,' Essay IV.). The confusion of the two conceptions expresses itself in the terminology of plant-breeding. Some writers define hybrid, for example, as a cross between species; this is the classificatory idea. Others define it to be any cross. The former use of the word is the more proper merely because it is the historic use, originating as a systematist's concept. The latter idea should have been expressed by a new word. It is for this reason that I have held to the old or systematic definition of hybrid; but there is no appeal against usage, so, while still proclaiming the righteousness of my cause as an easement of my conscience, I strike my colors and henceforth use the word hybrid for a cross of any kind or degree. How often does mere language confuse us!

From an argumentative point of view, it will be difficult to determine, in a given case, just what are variations and what mutations, for these categories are separated not by any quantitative or qualitative characters—the 'step' from one to the other may be ever so slight—but by the test that one kind is fully heritable and the other only partially so. If a mutation is to be defined as a heritable form, then it will be impossible to controvert the doctrine that evolution takes place by mutation, because the mutationist can say that any form that is inherited is by that fact a mutation. This will be equivalent to the position of those who, in the Weismannian days, denied the transmission of acquired characters, but defined an acquired character to be one that is not transmissible. However, it is to be hoped that the discussion of the mutation theory will not degenerate into a mere academic debate and a contention over definitions. Professor De Vries has himself set the direction of the discussion by making actual experiments the test of the doctrine. There will be confusing points, and times when we shall dispute over particular forms as to whether they are variations or mutations; but every one who has studied plants from the evolution point of view will be prepared to believe that species do originate by mutation. For myself, I am a Darwinian, but I hope that I am willing to believe what is true, whether it is Darwinian or anti-Darwinian. My own belief is that species do originate by means of natural selection, but that not all species so originate. De Vries's work will have a profound and abiding influence on our evolution philosophies.

II. HEREDITY: MENDEL.

De Vries made a thorough search of the literature of plant evolution. In an American publication he saw a reference to an article on plant hybrids by G. Mendel, published in 1865 in the proceedings of a natural history society of Brünn in Austria. On looking up this paper he was astonished to find that it discussed fundamental questions of hybridization and heredity and that it had remained practically unknown for a generation. In 1900 he published an account of it; and this was soon followed by independent discussions by Correns, Tschermak and Bateson. In May, 1900, Bateson gave an abstract of Mendel's work before the Royal Horticultural Society of England; and later the society published a translation of Mendel's original paper. It is only within the present year, however, that a knowledge of Mendel's work has become widespread in this country. Perhaps the two agencies that have been most responsible for dissemination of the Men-

delian ideas in America were the instruction given by Webber and others in the Graduate School of Agriculture at Columbus last summer, and the prolonged discussion before the International Conference on Plant-Breeding at New York last fall. Lately, several articles on the subject have appeared from our scientific press.

Mendel's work is important because it cuts across many of the current notions respecting hybridization. As De Vries's discussions call a halt in the current belief regarding the gradualness and slowness of evolution, so Mendel's call a halt in respect to the common opinion that the results of hybridizing are largely chance and that hybridization is necessarily only an empirical subject. Mendel found uniformity and constancy of action in hybridization; and to explain this uniformity he proposed a theory of heredity.

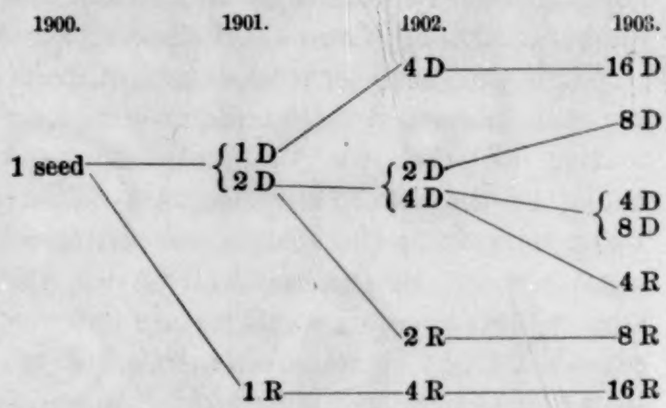
One of the most significant points connected with Mendel's work is the great pains he took to select plants for his experiments. He believed that hybridism is a complex and intricate subject, and that, if we are ever to discover laws, we must begin with the simplest and least complicated problems. He was aware of the general belief that the most diverse and contradictory results are likely to follow any hybridization. He conceived that some of this diversity may be due to instability of parents rather than to the proper results of hybridizing. He also saw that he must exclude all inter-crossing in the progeny. Furthermore, the progeny must be numerous, for, since incidental and aberrant variation may arise in the plants, it is only by a study of averages of large numbers that the true effects of the hybridizing are to be discovered. Moreover, the study must be more exact than a mere contrasting and comparing of plants: character must be compared with character.

The garden pea seemed to fulfill all the requirements. Mendel chose well-marked horticultural races or varieties. These he grew two years before the experiment proper was begun, in order to determine their stability or trueness to type. When the experiments were finally begun, he used only normal plants as parents, throwing out such as were weak or aberrant. Peas are self-fertile. It is to be expected that under such conditions the hybrid offspring would show uniformity of action; and it did.

In order to study the behavior of the hybrids, it was necessary to choose certain prominent marks or characters for comparison. Seven of these characters were selected for observation. These marks pertain to seed, fruit, position of flowers and length of stem, and they may be assumed to be representative of all other characters in the plant. These characters were paired (practically opposites) as long-stem *vs.* short-stem, round-seed *vs.* angular-seed, inflated-pod *vs.* constricted-pod. They were 'constant' and 'differentiating.' Of course every parent plant possessed one or the other of every pair of contrasting characters, but in order to facilitate his studies Mendel chose a different set of parents for each character, studying seed-shape in one set of hybrids, seed-color in another, pod-shape in another; in this way he avoided much complication in the results. Since it is not my purpose to discuss Mendel's work in detail, but only the general significance of its results, as they appeal to me, I need not describe these characters here. It will be sufficient for my purpose if I choose only one, the shape of the seed.

The seed-shape characters were roundness and angularity—the former being the 'smooth' pea of gardeners, and the latter the 'wrinkled' pea. Let us suppose that twenty-five flowers on round-seeded plants

were cross-pollinated in the summer of 1900 with pollen from angular-seeded plants, or *vice versa,* and that an average of four seeds formed in each pod. With the death of the parent plants the old generation ended, and the 100 seeds that matured in 1900—the year in which the cross was made—began the next generation; and these 100 seeds were hybrids. Now, all these 100 seeds were round. Roundness in this case was 'dominant.' (Dominance pertaining to the vegetative stage of the plant of course would not appear until 1901, when the seeds 'grow.') These seeds are sown in the spring of 1901. If each seed be supposed to give rise to four seeds—or 400 in all—this next generation of seeds (produced in 1901) will show 300 round and 100 angular seeds. That is, the other seed-shape now appears in one fourth of all the progeny; this character is said to have been 'recessive' in the first hybrid generation. If the 100 angular seeds, or recessives, are sown in 1902 it will be found that all the progeny will be angular-seeded or will 'come true'; and this occurs in all succeeding generations, providing no crossing takes place. If the 300 round seeds, or dominants, are sown in the spring of 1902, it will be found that 100 of them produce dominants only, and that 200 of them behave as before—one fourth giving rise to recessives and three fourths to dominants; and this occurs in all succeeding generations, providing no crossing takes place. In other words, the three fourths of dominants in any generation are of two kinds,—one third that produce only dominants and two-thirds that are hybrids. That is, there is constantly appearing from the hybrids one fourth part that are recessives, one fourth part that are constant dominants, and one half part that are dominants to all appearances, but which in the next generation break up again into dominants and recessives. This one half part that breaks up into the two characters consists of the true hybrids; but they are hybrids only in the sense that they hold each of the two parental characteristics—roundness and angularity—in their purity and not as blends or intermediates; and these two characteristics reappear in all succeeding generations in a definite mathematical ratio. Proportionally, these facts may be expressed as follows:



It will be seen that two thirds of the dominants break up the following year into one fourth constant dominants, one fourth recessives, and one half that again break up, the half that break up being the hybrids. This formula for the hybrids is Mendel's law. In words, it may be expressed as follows: Differentiating characters in plants reappear in their purity and in mathematical regularity in the second and succeeding generations of hybrid offspring of these plants; the mathematical law is that each character separates in each of these generations in one fourth of the progeny and thereafter remains true. In concise figures it is expressed as follows:

$$1D:2DR:1R.$$

$1D$ and $1R$ come true, but DR breaks up again into dominants and recessives in the ratio of 3 to 1.

Mendel found that this law holds more or less for the other characters that he

studied in the pea, as well as for the seed-shape. He did not conclude, however, that it holds good for all plants, but left the subject for further investigation. He himself found different results in *Hieracium*. It will be seen at once that it will be a very difficult matter to follow this law when many characters are to be contrasted, particularly when the characters are merely qualitative and grade into each other. The dominant characters pertain to either parent: some of them may come from the mother and some from the father.

When this roundness is dominant from the male parent, it falls under the denomination of what we commonly know as xenia, or the immediate effect of pollen; when it is from the female parent, there is no xenia. In the case of the pea, the seed-content is embryo and we are not surprised if there is xenia. In those plants in which the embryo is embedded in endosperm, however, it would seem to be difficult to account for xenial dominance, unless there is double fecundation, as appears to be the case in Indian corn, as pointed out by De Vries, Webber* and others. It looks as if the question of dominance would introduce a new point of view into the study of xenia. There is now a strong tendency, however, to use the word xenia to designate only those effects occurring outside the embryo.

Which characters will be dominant in any species we cannot determine until we perform the experiment; that is, there is no mark or attribute which distinguishes to us *a priori* a dominant or a recessive character. However, the mere fact as to whether the one or the other character is dominant is relatively unimportant, for constant dominance is no more a regular behavior than recessiveness is. In various subsequent experiments it has been found that even when marked dominance is not shown in the first product, the hybridization may follow the law in essential numerical results. The really important points are two: (1) that the characters typically remain pure or do not bend, (2) and that their reappearance follows a numerical order.

After finding such surprising results as these, Mendel naturally endeavored to discover the reasons why. The product of his speculations is the theory of gametic purity (to use our present-day terminology), which is a partial theory of heredity. Every plant is the product of the germ cell fertilized by the sperm cell. When constant progeny is produced, it must be because the two cells, or gametes, are of like character. When inconstant progeny is produced, it must be because the sperm cell is of one character and the germ cell of another. When these unlike gametes come together, they will unite according to the law of mathematical probabilities, one fourth of those of each kind coming together and one half of those of both kinds coming together. If A and B represent the contrasting parental characteristics, they would combine as

$$A + A = A^2$$
$$A + B = AB$$
$$B + A = BA$$
$$B + B = B^2$$

A^2 and B^2 are equivalent only to A and B. Since both of the opposed or contrasted characters can not be visible at the same time, we have the following:

$$A$$
$$A^b$$
$$A^b$$
$$B$$

in which small b represents the character that for the time being is not able to express itself, or is recessive, and large B represents the same character fully expressed.

* 'Bull. 22, Div. of Veg. Phys. and Path.,' U. S. Dept. Agric., 1900.

In these gametes, the unit characters of the plants that bear them are pure. Even in hybrid plants, the pollen grains and the egg cells are not hybrids. According to this hypothesis of gametic purity, therefore, hybrids follow natural and numerical laws; but these laws are always obscured by new crossing. True intermediate characters do not occur. If new characters appear, it is because they have been recessive or latent for a generation or because the plant has varied from other causes: they are not the proper results of hybridization. Possibly new characters that appear because of effect of environment or other cause may be impressed on the gamete and thereby be perpetuated. The results of hybridization, then, according to the Mendelian view, are not fundamentally a mere game of chance, but follow a law of regularity of averages; but the results are so often masked that it is sometimes impossible to recognize the law.

Mendel's law of heredity is recently stated as follows by Bateson and Saunders: 'The essential part of the discovery is the evidence that the germ-cells or gametes produced by cross-bred organisms may in respect of given characters be of the pure parental types, and consequently incapable of transmitting the opposite character; that when such pure similar gametes of opposite sexes are united together in fertilization, the individuals so formed and their posterity are free from all taint of the cross; that there may be, in short, perfect or almost perfect discontinuity between these germs in respect of one of each pair of opposite characters.'

This, in barest epitome, is the teaching of Mendel. This teaching strikes at the root of two or three difficult and vital problems. It presents a new conception of the proximate mechanism of heredity, although it does not present a complete hypothesis of heredity, since it begins with the gametes after they are formed, and does not account for the constitution of the gametes nor the way in which the parental characters are impressed upon them. This hypothesis will focus our attention along new lines, and I believe will arouse as much discussion as Weismann's hypothesis did; and it is probable that it will have a wider influence. Whether it expresses the actual means of heredity or not, it is yet much too early to say; but this hypothesis is a greater contribution to science than the so-called 'Mendel law' as to the numerical results of hybridization; the hypothesis attempts to explain the 'law.'*

One great merit of the hypothesis is the fact that its basis is a morphological unit, or at least an appreciable unit, not a mere imaginary concept. This unit should be capable of direct study, at least in some of its phases. It would seem that the Mendelian hypothesis would give a new direction to cytological research.†

It is yet too early to say how far Mendel's law applies. We shall need to restudy the work that has been done and to do new work along more definite lines. There are relatively few results of experiments that can be conformed to Mendel's law, because the data are not complete enough or not made from the proper point of view. We should expect the fundamental results to be masked when the plants with which we work are themselves unstable, when cross-fertilization is allowed to take place, or when the pairs of contrasting characters are very numerous and very complex. Marked numerical results

* This, I take it, is also the opinion of Bateson, the leading interpreter of Mendel in English; for he calls his new book on the subject (1902) 'Mendel's Principles of Heredity,' as if the heredity idea were greater than the hybridization idea.

† See, for example, 'A Cytological Basis for the Mendelian Laws,' *Bull. Torr. Bot. Club*, 29, 657 (1902), by W. A. Cannon.

have been found by various workers in different fields, in this country notably by Spillman in hybrid wheats. Mendel was able to discover the numerical law because he eliminated nearly all of the confusing contingencies. In the discussion of every bold new hypothesis, we are in danger of becoming partisans, taking a stand either for it or against it. The judicial attitude is also the scientific one. We want to know.

Two processes are now going forward in the discussion of Mendel's law—one the explaining away of 'exceptions,' the other the endeavoring to find the true place of the law in the scheme of evolution. The one is primarily an effort to uphold the law; the other is primarily a desire to adjudge it. One is an effort to apply it universally; the other to determine whether it is universal. Already so many adjustments have been made of the Mendelian principles that it is becoming difficult to determine what Mendelism is. These cases are typical of the discussions on almost every vital question connected with evolution. At the hard places we make a supposition and modify the hypothesis in the face of a fact. We can prove anything by supposing.

The results of Mendel's work have two important bearings on current evolution discussion: (1) on the part that hybridization plays under natural conditions in the evolution of the forms of life, and (2) the part that it plays in plant-breeding. In the former category, Mendel's work gives a hint of definiteness to the rôle of hybridization in the origination of new combination-forms. In the latter category, it is difficult as yet to measure its importance, since extended applications to practice have not been made and since, also, the Mendelian principles have been so much extended and redefined within the past two years that it is difficult to determine just what is Mendelism and what is an endeavor to make the Mendelian suggestions fit our present-day knowledge. In discussing the application of Mendel's work to plant-breeding, I desire to keep in mind the work that he did with peas, upon which the 'Mendel law' chiefly rests.

III. APPLICATION TO PLANT-BREEDING.

The wildest prophecies have been made in respect to the application of Mendel's law to the practice of plant-breeding, for the mathematical formulæ express only definiteness and precision. Unfortunately, the formulæ can not express the indefiniteness and the unprecision which even Mendel found in his work. My own feeling is that the greatest benefit of Mendel's work to the plant-breeder will be in improving the methods of experimenting. We can no longer be satisfied with mere 'trials' in hybridizing: we must plan the work with great care, have definite ideals, 'work to a line,' and make accurate and statistical studies of the separate marks or characters of plants. His work suggests what we are to look for and new ways of attacking difficult problems.

Beyond this, I do not see how the original Mendelian results will greatly modify our plant-breeding practice. The best breeders now breed to unit characters, for this is the significance of such expressions as 'avoid breeding for antagonistic characters,' 'breed for one thing at a time,' 'know what you want,' 'have a definite ideal,' 'keep the variety up to a standard.' In certain classes of plants the Mendelian laws will be found to apply with great regularity, and in these we shall be able to know beforehand about what to expect. The number of cases in which the law, or some modification of it, applies is being extended daily, both for animals and plants (see, for example, Bateson and Saunders' report to the Royal Society on heredity);

but in practice we shall probably find many more exceptions to the formulæ than confirmations of them, even though the exceptions can be explained, after we find them, by Mendel's principle of heredity.

It has been said that we shall soon be able, as a result of Mendel's discoveries, to predict varieties in plant-breeding. Before considering this question, we must recall the fact that a cultural variety is a succession of plants that have characters sufficiently marked and uniform to make it worth cultivating in place of some older variety. Now and then it may be worth while to introduce some new energy or new trend into a general lot of offspring by making wholesale crosses, not expecting ever to segregate any particular variety or strain from the progeny; but these cases are rare, and the gain is indefinite and temporary. So far as our knowledge at present goes, I see no warrant for the hope that we can predict varieties with any degree of exactness, at least not beyond a very narrow effort. Following are some of the reasons that seem to me to argue against the probability of useful prophecy of varities, so far as the Mendelian results are concerned: (1) We do not know what plants will Mendelize until we try. (2) Even in plants that do Mendelize, only half of the offspring have stable characters. But we can not predict for even this half, for it is impossible to determine beforehand which seeds showing dominant characters (and these are three fourths of the offspring) will 'come true.' Dominance, as we have seen, is of two kinds in respect to its behavior in the next generation—constant and hybrid; and the hybrid dominance, which is twice as frequent as the other, breaks up into constant dominance, hybrid dominance and recessiveness. (3) Mendel's law deals primarily with mere characters, not with a variety or with a plant as a whole. Every plant is a composite of a thousand characters, and from the plant-breeder's point of view there may be as many undesirable characters as desirable ones. No plant is perfect; if it were there would be no need of plant-breeding. The breeder wants to preserve the desirable characters or traits and eliminate the undesirable ones, but under the strict interpretation of Mendelism this is difficult. The one germ gamete and the one sperm gamete that unite to make the new plant each contain all the alternative characters; these characters are bound to reappear in the offspring, and all that the breeder gains is a new combination or arrangement of characters, and undesirable attributes may be as troublesome as before. (4) The breeder usually wants wholly new characters as well as recombinations of old ones, or he wants augmented characters. For example, a carnation grower wants a four-inch flower, but he has only three-inch flowers to work with, and augmentation of characters is no part of the original Mendelian law. Perhaps these augmented and new characters are to be got by means of ordinary variation and selection, or other extra-crossing means; but we know, as a matter of fact, that augmented characters do sometimes appear in hybrids. (5) New and unpredictable characters are likely to arise from the influence of environment or other causes, and these may be recorded in the gametes and vitiate the final results. (6) Variability itself may be a unit character, and therefore pass over. There is probably such a thing as a 'tendency to vary,' wholly aside from the fact of variation. (7) Many of the plants with which we need most to work in plant-breeding are themselves eminently variable, and the results, even if there is true Mendelism, may be so uncertain as to be wholly unpredictable. (8) Many plants with which we must work will not close-fertilize. Some

of them are monœcious or diœcious. Even if there is gametic purity in such plants, the probability is that the fact can be discovered only by a long line of scientific experimenting for that particular purpose, and not by the work of the man who desires only to breed new plants. (9) A cultural variety, in any true acceptation of the term, is a series of closely related plants having a pedigree. It runs back to one individual plant, from which propagation has been made by seeds or asexual parts. Now, one can never predict just what combination of characters any plant will have, even though it be strictly Mendelian. A person might have a thousand plants of peas of which no one plant shows any of the characters in the proportion of 3 to 1, let alone all the characters as 3 to 1; and yet the total average numerical results might conform exactly to the Mendelian law. Mendel's law is a law of averages. The very fact that one must employ such large numbers to secure the numerical results shows that we can not predict as to individuals. For example, in ten plants of pea, Mendel found the following ratios in respect to seed-shape and seed-color:

Shape.	Color.
3.75:1	2.27:1
3.37:1	4.57:1
3.43:1	2.80:1
1.90:1	2.59:1
2.91:1	1.85:1
4.33:1	3.33:1
3.66:1	2.43:1
2.20:1	4.88:1
4.66:1	3.57:1
3.57:1	2.44:1

Mendel reports one instance in which the ratio in seed-shape was 21 to 1, and another of 1 to 1. He also reports instances of seed-color of 32 to 1 and 1 to 1. It has been said that, because of Mendel's work, we shall be able to produce hybrid varieties with the same certainty that we produce chemical compounds. Now, a plant is made up of many combinations of many units, and these combinations are the results of mathematical chance or probability. Chemical compounds are specific entities, in which the parts combine by mathematical definiteness. The comparison, as it appeals to me, is fallacious and the conclusions unsound.

We must remember that there are whole classes of cases of plant-breeding that do not fall under hybridization at all. Granting the De Vriesian view that selection is incompetent to produce species from individual fluctuations, it is, nevertheless, well established (and admitted by De Vries) that very many of our most useful cultural varieties have been brought to their present state of perfection by means of selection; and by selection they are maintained in their usefulness. Selection will always be a most important agency in the hands of the gardener—none the less so now that we have challenged its rôle in the evolution of the plant kingdom. For the time being, the new discussions of hybridization are likely to overshadow all other agencies in plant-breeding; but selection under cultivation is as important now as it was in the days of Van Mons and Darwin.

IV. INTERPRETATION OF HYBRIDISM.

I believe that the clearest insight into this whole new question of hybridization is to be got by following the work of De Vries. The concluding parts of the second volume of his 'Mutationstheorie,' a volume devoted wholly to hybridization, is on the press at this moment. The Mendelian laws are fully discussed in this volume, but the summary conclusions may be presented here. De Vries had been working at hybridization long before he discovered Mendel, and had arrived at practically the same results; he had also arrived at other results

that are not Mendelian. De Vries denominated the law of numerical segregation as the 'law of separation of characters in crosses.' Like Mendel, he had found that merely to cross 'varieties' or 'species' is of no avail in the study of fundamental problems; for the varieties and species that we know are mere systematic groups with characters of all kinds and degrees. We must cross characters or units, not species.

Now, every unit character he conceives to be represented in the germ by a pangene. This pangene may be active, in which case the character appears in the plant; or it may be dormant, in which case the character is not visible, or for the time being is lost. Active pangenes may at any time become latent, or latent ones may become active.

Mendel's law results from an interchange of contrasting characters. True physiological or elementary species differ from each other by new unit characters. They have arisen by progressive mutation. The characters are not contrasting or differentiating. One species has one kind of pangene, another species another kind of pangene. On combining these there can be no interchange of characters, and therefore no Mendelism. There is nothing for one character to exchange against the other. In the case of true progressive mutations, therefore, upon which the progress of the plant race depends, there can be no Mendelizing. Hybrids of these cases are intermediates, or else follow only one or the other of the parents.

Now, varieties differ from true mutative species in the fact that they have contrasting characters. These characters are represented by their special kinds of pangenes. The pangene may be active or passive. That is, the variety may be a variety because one or more of its characters has become latent (retrogressive) or because characters have become active (degressive). When these characters are crossed, there is an interchange of the pairs. Both parents bear the same unit character, but this character is active in the one and dormant in the other. The hybrid receives an active pangene from one parent and a similar but inactive pangene from the other. When these two units unite, the calculus of chance determines that there shall reappear in the second generation equal numbers of both the parental units, and half of the whole that are still hybrids and break up in the same ratio in the third generation. That is, true Mendelism is confined to crossings of retrogressive and degressive varietal characters.

There are, therefore, two general classes of hybrid formation—the isogons, giving rise to crosses in which two antagonistic parental characters reappear in numerical order (Mendelian cases); anisogons, giving rise to crosses in which two antagonistic sometimes separate unequally, but ordinarily do not separate at all. When only one parent is represented in the offspring, we have the 'unisexual crosses' of Macfarlane or the 'false crosses' of Millardet. These are cases in which there are no true contrasting characters. Spillman has recently explained the false hybrids by supposing that the plants in this case are self-fertile and sterile with other pollen. That is, *A* is fertile with *A*, *B* with *B*, but *A* is not fertile with *B* nor *B* with *A;* there results, therefore, no true crossing. This hypothesis should be capable of experimental proof or disproof.

The isogon hybrids are of all degrees of complexity, and classification of them will at once show how far we have already got away from the old systematic idea of variety-hybrids and species-hybrids. Hybrids between plants that differ only in one unit-character are monohybrids. These

are the ones in which the numerical results are most clearly traced, but they are also exceedingly rare. Those in which two unit characters are concerned are dihybrids. In these the combination series gives four different kinds of offspring. So there are trihybrids, giving eight possible combinations, tetrahybrids, and so on to polyhybrids; and in every succeeding grade the difficulties of statistical and comparative studies increase. Of how many characters is a plant composed?

V. CONCLUSION.

Now, in conclusion, what are the great things that we have learned from these newer studies? (1) In the first place, we have been brought to a full stop in respect to our ways of thinking on these evolution subjects. (2) We are compelled to give up forever the taxonomic idea of species as a basis for studying the process of evolution. (3) The experimental method has finally been completely launched and set under way. Laboratory methods, comparative morphology, embryological recapitulation, life history studies, ecological investigations—all these means are likely to be overshadowed for a time by experiments in actually growing the things under conditions of control. (4) We must study great numbers of individuals and employ statistical methods of comparison. (5) The doctrine of discontinuous evolution is now clearly before us. (6) We are beginning to find a pathway through the bewildering maze of hybridization.

L. H. BAILEY.

CORNELL UNIVERSITY.

THE SOCIETY FOR PLANT MORPHOLOGY AND PHYSIOLOGY.

THE sixth regular annual meeting of this society was held, in conjunction with the meetings of the American Society of Naturalists and the American Association for the Advancement of Science, at Washington, December 30 and 31, 1902, under the presidency of Professor Volney M. Spalding. A large part of the members were in attendance, and the meeting was in all ways most successful and pleasant. New members were elected as follows: Messrs. W. A. Cannon, of the New York Botanical Garden; Judson F. Clark, of Cornell University; G. P. Clinton, of the Connecticut Agricultural Experiment Station; W. C. Coker, of the University of North Carolina; C. C. Curtis, of Columbia University; E. J. Durand, of Cornell University; J. E. Kirkwood, of Syracuse University; W. A. Orton, of the United States Department of Agriculture, and K. M. Wiegand, of Cornell University. The following officers were elected for the ensuing year:

President—Roland Thaxter, of Harvard University.

Vice-President—Conway MacMillan, of the University of Minnesota.

Secretary-Treasurer—W. F. Ganong, of Smith College.

The chief item of business of general interest was the discussion upon the practicability and desirability of the new Central Bureau 'for the obtaining and distribution of material for investigation and demonstration' proposed by the Association Internationale des Botanistes. An expression of opinion taken after the discussion showed a unanimous opinion against the plan. Suggestions were formulated towards securing further improvements in the *Botanisches Centralblatt,* and a committee was appointed to draw up and publish in SCIENCE and elsewhere a statement to American botanists of the desirability of giving their full support to the *Centralblatt,* and of declining to support a competing journal.

The social features of the meeting were of unusual attractiveness. The society joined with the other societies in the vari-

ous public entertainments which had been arranged by the American Association, and in addition two notable courtesies were extended to the visiting members of the society by botanists of Washington—the first, a charming dinner at the Hotel Barton, given to the visiting members of the society and their wives by the Washington members and their wives on Tuesday evening, and a reception later the same evening given to all the visiting botanists by the Botanical Society of Washington.

The address of the president, Professor Volney M. Spalding, dealt with 'The Rise and Progress of Ecology,' and was delivered after the dinner at the Hotel Barton. It is believed to be the first presidential address to deal with this subject. It was published in full in this journal for February 6.

The society voted to extend its warmest thanks to the authorities of Columbian University, to its members resident in Washington and to the Botanical Society of Washington, for the many courtesies which had contributed to make the meeting so successful and enjoyable.

Following are abstracts of the papers actually presented in full before the society and thrown open for discussion, excluding those offered by title. The abstracts are by the authors. Certain papers appear by members of the new Association of Botanists of the Central States, the sessions being to some extent joint ones with that association.

A Discussion of Mendel's Law and its Bearings: Professor L. H. Bailey, Cornell University, and Dr. Herbert J. Webber, Department of Agriculture.

Professor Bailey's paper is published above. It is expected that Dr. Webber's paper will also be published in this journal.

The Early Root Development of Tree Seedlings, an Important Factor in their Local Distribution: Professor J. W. Toumey, Yale Forest School.

A series of twenty slides were shown of the initial root systems of various root types of seedlings of American trees, photographed in various stages of germination, and at different later periods until the species had grown well-developed initial root systems. All of the seedlings were grown at approximately the same time and under the same soil and atmospheric conditions.

From the study of the root systems of the various species, it appeared that the form of the initial root systems of the trees studied is surprisingly constant for the same species. In other words, there is an inherent tendency for each species to produce an initial root system that takes a definite form and direction. Early in the life of the seedling, this initial root system becomes more or less modified by its environment, particularly by the moisture and other soil factors. It was shown that there are great differences in the different species studied in the plasticity of the initial root system; that is, in the rapid and marked changes from its initial form and characteristics under the influence of environment. In some of the species shown, as in many hickories and oaks, the initial root system has remarkable fixity. The general form of the initial root persists, no matter upon what soil the species grows. It was pointed out that the species which show but little plasticity in the initial root system under the influence of environment, do not readily adapt themselves to variable soil moisture conditions.

In others of the species shown the initial root system is extremely plastic, rapidly changing under environmental influence, as illustrated in the red maple. In this species the initial root system consists of

the long tap-root and a few strong lateral roots very near the surface of the soil. In wet situations the tap-root soon disappears, and the plant becomes surface-rooted from the development of the lateral roots. In a dry situation the tap-root persists and the initial lateral roots disappear. Trees exhibiting this plasticity readily adapt themselves to a great diversity of situations as to soil and moisture. Thus we find the red maple grows in swamps, and also on dry rocky ridges.

It was further shown that the form and behavior of the initial root system, in its development prior to its becoming materially modified under the influence of environment, is directly correlated with the soil moisture conditions best suited to its growth and development. It appears possible to classify our woody plants into groups based upon differences in form and development of their initial root systems, and their plasticity under the influence of environment, and judge, with a fair degree of accuracy, the locality as to soil moisture best suited to each group.

Observations on a Hitherto Unreported Bacterial Disease, the Cause of which enters the Plant through Ordinary Stomata: Dr. ERWIN F. SMITH, Department of Agriculture.

A disease of Japanese plums of unusual interest has made its appearance in central Michigan. It is first visible in the form of numerous small water-soaked spots on the leaves and green fruits. The leaf disease ends in 'shot-holes'; the fruit disease ends in roundish, sunken, shallow black spots and in deep fissures which spoil the plums. The spots enlarge slowly, but may finally reach a diameter of one fourth to one half inch. The disease is due to a yellow bacterium, *Pseudomonas pruni* Smith, *which enters the uninjured plant through ordinary stomata.* In the earliest stage of the disease, visible only under the compound microscope in properly fixed and sectioned material, the bacteria are confined to the substomatic chamber. From this point they push into the deeper tissues, and by the time the spots have become large enough to be seen under a hand lens (as small water-soaked areas—one fifth to one half mm. in diameter), the bacteria have multiplied enormously in the depths, pushing up the epidermis and the cells immediately under it, and forming in the deeper tissues closed cavities of considerable size. Later, when the enlarged spots have begun to sink in and become brown, the bacteria reach the surface as numerous tiny, rounded, pale-yellow, gum-like masses, which ooze from the stomata lying over the closed bacterial cavity. The infections take place principally in May and June and no wounds are necessary. The shaded and west side of the fruits are most subject to infection, *i. e.*, those on which the rain drops or dew drops (necessary for infection) would persist longest because best protected from the morning sun. This is primarily a disease of the parenchyma, but the bundles are finally invaded.

The organism is distinctly yellow and grows readily in ordinary culture media, bouillon, milk, potato, agar, etc. It was easily obtained in pure culture from small spots. In agar plate cultures it looks much like *P. campestris,* but is readily distinguished by its feebler growth on potato and by its behavior in Uschinsky's solution, which is converted by it from a limpid fluid to one as viscid as egg albumen. The bacteria are small to medium size and occur singly, in pairs, or short chains. They are motile by means of one to several polar flagellæ. The thermal death point is approximately 51° C. Gelatin is not liquefied rapidly. Litmus in milk is reduced, but finally returns to its

former color. Casein is slowly precipitated and finally redissolved. No gas is produced from any medium.

The paper was illustrated by fifty lantern slides showing the location of the bacteria in the tissues and illustrating the morphology and cultural characters of the organism.

Completed Proof that P. Stewarti is the Cause of the Sweet Corn Disease of Long Island: DR. ERWIN F. SMITH, Department of Agriculture.

In the winter of 1897–98, Stewart described a disease of sweet corn from Long Island which he attributed to a yellow bacterium that was very abundant in the vessels. This organism Smith subsequently named *P. Stewarti.* Stewart's infection experiments were inconclusive partly because made in a locality where the disease occurred naturally and soon appeared on the check plants, and partly because not made in the most natural way.

In the summer of 1902 the writer visited Long Island and obtained pure cultures of the organism. With these about 500 sweet corn plants of several varieties were inoculated, all during the seedling stage. Part of these plants were exposed to infection by placing the bacteria in drops of fluid oozing from the water-pores at the tip of the leaf, part by shaking up slant-agar cultures in sterile water and spraying this on the plants in a fine mist, during the period when they were extruding water from their leaf-tips. Both methods yielded good results. The first shriveling of tissue was at the tips of the inoculated leaves. Typical constitutional symptoms appeared in a few plants during the first month, but most of the cases developed the second and third month when the plants were several feet high. In such it was common to find the vascular system plugged with this yellow bacterium in practically pure culture all the way from the basal nodes to the top of the plant, four and one half feet in some cases. The nodes of such plants were browned inside very decidedly, especially the basal ones; the internodes within were generally white, with yellow bundles from which the bacteria oozed abundantly on cross-section. Frequently 150 or more bundles would be occupied. More than 300 typical cases of this disease were obtained, and many other plants would undoubtedly have shown symptoms had not the experiment been cut short by a frost. One of the first symptoms of this disease is the whitening and death of the male inflorescence. The leaf blades dry out one after another until all are dry, while the stem is still green. In this condition the affected plants look as if frosted, and the cause of the disease is not apparent until the plants are cut open.

This experiment, conducted in Washington, where the disease does not occur, shows conclusively that wounds are not necessary for infection, *and makes it reasonably certain that natural infections take place as a rule through the water-pores or ordinary stomata in the seedling stage of the plants.* The vascular system is the primary seat of the disease, but small cavities filled with the bright yellow slime finally appear in the parenchyma. The bacteria were not confined to the stem but passed out into the vascular system of the blades of the middle and upper leaves and into vessels of the husks and cobs. The paper was illustrated by lantern slides.

Opportunities for Study at the Minnesota Seaside Station: Professor CONWAY MACMILLAN, University of Minnesota.

The speaker gave an account of the surroundings of the station, the particularly rich marine flora, and the advantages offered for investigation in this compara-

tively new field, illustrating the subject fully by lantern slides.

On the 'Blue' Color of Coniferous Timber: Dr. Hermann von Schrenk, Missouri Botanical Garden.

Following an attack of the destructive pine bark beetle in South Dakota, the sapwood of the bull pine (*Pinus ponderosa*) turned blue. The color first appears at the base of the tree some months after the beetle attack, and gradually spreads up the trunk until it has reached the top. The color is evenly distributed throughout the sapwood, and is very permanent. Reference was made to the researches of Vuillemin on the 'green' color of wood, which he found due to a substance, *xylindeine*, formed by *Helotium aeruginascens*. The 'blue' color of pine wood is due to the growth in the wood of *Ceratostomella pilifera*, the fruiting bodies of which grow on the outside of affected wood. The life history of the fungus was described and cultures exhibited. No coloring matter could be extracted from 'blue' wood, and it is probable that the color is largely due to the blending of the brown color of the fungus present throughout the 'blue' wood, with the color of the wood itself. The 'blue' wood was shown to be as strong mechanically as green wood.

The Development of the Prothallium in Pinus: Dr. Margaret C. Ferguson, Wellesley College.

A few of the more important conclusions reached in a detailed study of the development of the prothallium in *Pinus* were given.

The ovules are not differentiated in the species of pines studied until about three weeks before pollination.

The macrospore-mother-cell may originate from a hypodermal cell as ordinarily stated, but in a study of the development of the ovule there is not the slightest evidence of such an origin.

The first division of the macrospore-mother-cell is heterotypical in nature and gives rise to the one half number of chromosomes. This division is quickly followed either in the lower cell only, or in both cells, by a homotypical division, thus giving rise to axial rows of three or four cells. The basal cell results from a true tetrad-division, and always forms the functional macrospore.

The macrospore passes through a period of growth lasting about six weeks. During this time the peripheral layer of cytoplasm is organized and the nucleus takes up a position in the wall-layer of cytoplasm near the micropylar end of the cell.

Thirty-two free nuclei are formed before the approach of winter, and more than two thousand nuclei have been counted at the time when cell-walls are laid down. In the later development of the prothallium, true 'alveoli' are not formed, but each cell divides several times before reaching the center of the prothallial cavity.

The 'spongy tissue' is not disintegrating tissue, as previously stated, but it forms a zone of physiological tissue which plays an important part in the nutrition and support of the developing gametophyte.

Fertilization in Taxodium: Professor W. G. Coker, University of North Carolina.

The male gametophyte of *Taxodium* is much like that of the Cupresseæ. No sterile prothallial cell is formed, and the pollen-tube reaches the archegonia before the division of the central cell occurs. This division takes place simultaneously with the ventral-canal division in the archegonium, and in a day or two fertilization is completed. The sperm-cells are of equal size, and, as a rule, each is instrumental in fertilizing an archegonium. In outline the sperm-cells resemble those of

the Cupresseæ and *Sequoia*. They are sharply separated from the protoplasm of the pollen-tube by a distinct hautschicht. Immediately around the nucleus is a thick layer of starch; next to this peripherally comes an imperfect layer of granular material staining red in saffranin which is probably of the same nature as the large masses found in the spermatozoids of *Ginkgo*. Between this layer and the hautschicht is a narrow zone of clear protoplasm without appreciable inclusions. In the Abieteæ the pollen-tube contains starch, but there is none in the sperm-cells themselves, while in *Taxodium*, on the contrary, the pollen-tube is free from starch and the sperm-cells are loaded with it.

The ventral-canal division in the archegonium does not produce a ventral-canal cell, separated from the egg, but simply cuts off a ventral-canal nucleus which, while closely pressed to the surface of the egg, is still included in its protoplasm. This nucleus is rarely cut off at the very tip of the egg, but is generally lateral in position and may even be half way down the side. It does not degenerate at once, but persists until after fertilization, and then generally divides amitotically.

In fertilization the entire sperm-cell enters the egg, passes through its protoplasm, and comes in contact with the egg nucleus, around which it folds. The starch is thus distributed evenly around the fusion nucleus and sinks to the base of the archegonium with it, to be included in the small amount of protoplasm cut off at the base of the egg as the proembryo. In *Taxodium*, then, almost the whole of the included food material and a considerable part of the protoplasm of the embryo are derived from the sperm-cell, while only a small part of the protoplasm of the egg is instrumental in embryo formation, the remainder being digested and absorbed by the young plant. Such a type of fertilization is known so far only in the group Taxodieæ.

Stamens and Pistils are Sexual Organs: Professor W. F. GANONG, Smith College.

The author contended that the current effort to restrict the sex-terminology to the gametophyte in the flowering plants is misdirected, for not only does the sex-terminology belong to stamens and pistils on the ground of priority, but also as a matter of physiological fact. The paper will appear in full in this journal.

The Toxic Effects of Some Nutrient Salts on Certain Marine Algæ: Professor BENJAMIN M. DUGGAR, University of Missouri.

Some work conducted in part at the Naples Marine Biological Laboratory and in part at Woods Holl was reported. In attempting some osmotic studies with solutions of cane sugar, potassium nitrate and sodium chloride, it was found that with isotonic solutions, either in sea water or in distilled water, the results were unusually inconsistent. It seemed probable that the explanation might be found to be connected with the toxic action of the salts used. Accordingly, an investigation was attempted of the toxic action of certain nutrient salts found in sea water when added to sea water, other chemical agents being also used for comparison. Seven algæ were employed, namely, *Chætomorpha linum, Cladophora gracilis, Dasya elegans, Pleonosporium coccinium, Grinnellia Americana, Griffithsia Schousberi* and *G. opuntioides*. It is desirable to employ for such studies algæ which change color as soon as killed, or those with which the plasmolytic test may be readily employed.

After the acids and some of the salts of the heavy metals, the potassium phosphates proved most toxic; and the latter are closely followed by the neutral salts of ammonium, among which the sulphate is

most injurious. The last-mentioned are followed by some salts of potassium and calcium in irregular order, although it is to be noted that potassium nitrate is about twice as poisonous as potassium chloride. The least toxic are the salts of sodium and magnesium. An average of the experiments shows magnesium sulphate to be the least toxic of all salts which have been used as sulphates, chlorides or nitrates. The low toxicity of the magnesium salts with relation to the marine algæ makes it evident that these plants are very notable exceptions to the rule which Loew and others found to hold for many phanerogams and fresh-water algæ.

The toxicity of the salts studied bore no close relation to the relative amounts of these salts normally present in sea water. The inconsistent results with potassium nitrate and other salts as plasmolytic agents may be partially explained by the toxic action of these salts on the marine algæ.

The Nature and Function of the Pyrenoid: Mr. H. G. TIMBERLAKE, University of Wisconsin.

Among the structures found in the cells of the green algæ the pyrenoid occupies a doubtful position. The question as to whether it is to be considered a true cell-organ or a mere mass of reserve material is partly, but not wholly, solved by its history in connection with the various phases of the life history of the cell and its relation to the process of starch formation. That the pyrenoid may be reproduced by division is shown in the cells of *Cladophora*, *Œdogonium* and other filamentous algæ, as also in *Chlamydomonas* among the unicellular forms. In this latter case the pyrenoid divides during the division of the cell. On the other hand, it is well established that under ordinary circumstances the pyrenoids entirely disappear prior to spore formation in *Hydrodictyon* and are afterward formed anew in the young cells.

The relation of the pyrenoid to starch formation in *Cladophora* and other forms studied is essentially the same as that already described by the author for *Hydrodictyon* (*Annals of Botany*, December, 1901). The following additional details are noted: The usual shape of the pyrenoid in the species of *Cladophora* studied is that of a biconvex lens. The differentiation of the pyrenoid into two parts takes place in such a way as to divide it by a plane passing through its longer axis. In many cases the pyrenoid is actually split into halves with a fairly broad cleft between them. Either of the halves so formed may be converted into a starch grain. In some instances the entire pyrenoid is converted into starch without previous cleavage. This is more apt to happen in *Œdogonium* and *Rhizoclonium* than in *Cladophora*.

The nature of the chemical processes involved in the formation of starch from a pyrenoid is now under investigation. That the process involves a conversion of a proteid substance (the pyrenoid) into a carbohydrate (starch) seems reasonably certain, but the unreliable character of various microchemical reactions makes the study of the details very difficult.

Observations upon the Morphology of a Species of Osmunda from the Cretaceous Formation, and its Relation to Existing Species: Professor D. P. PENHALLOW, McGill University.

In material from the Cretaceous of Skidegate Inlet, Queen Charlotte Islands, collected by Dr. C. F. Newcombe in 1895 and 1897, there were several fragments of plants representing the stipe, rhizome, fertile and sterile pinnules of a fern. Although not in actual connection, these fragments proved, upon examination, to belong to the same genus and undoubtedly

to the same species which has, therefore, been designated as *Osmundites skidegatensis.* From the material now at hand it is possible to effect a complete restoration of the plant with the exception of details relating to the sporangia, but as these structures differ but little in the Osmundaceæ, it would be possible to complete even this detail in a general way, from existing types. A close comparison with existing representatives of this family shows that it approaches the type of *Todea* in certain details of the phloem structure, as also in the absence of an endodermal layer. In all other respects it closely approaches the type of *Osmunda,* to which it is no doubt most closely related. A fact of very special interest is derived from a close comparison of the relative dimensions of the various structural regions and organs, from which it appears that the fossil must have been at least eight times larger than the modern *Osmundas* such as *O. claytoniana* or *O. cinnamomea,* and, with respect to the individual stem, much larger than *Todea barbara.* The general conclusions which these facts seem to indicate are that *Osmundites* represents a transitional form from which, or from a point near which, divergent lines of development arose, leading to the type of *Todea* on the one hand, and to the type of *Osmunda* on the other. It would also seem that *Osmundites* must have represented a period when the individual members of the family were much larger that at present, the existing species indicating, in their small size and diminished numbers, a tendency toward obliteration of this branch. The paper was fully illustrated by lantern slides.

Ecological Conditions of Plant Growth in the Isle of Pines: Professor W. W. ROWLEE, Cornell University.

The Isle of Pines has an area of nearly a thousand square miles. It is about one fifth as large as Jamaica and is as large as all the other islands that immediately surround Cuba would be if put together. It lies about thirty miles south of western Cuba, from which it is separated by a very shallow archipelago, the islands of which are small coral keys covered, for the most part, with a dense growth of mangrove. The island lies on the southern verge of the plateau, the northern part of which is the island of Cuba.

The northern and larger part of the island consists of a rolling table-land, in its highest parts scarcely more than 300 feet above tide. The most conspicuous physiographic features of this part of the island are the mountains which rise abruptly in isolated masses to a height of from 500 to 1,600 feet. The principal ones are Sierra Canada, Sierra Caballos and Sierra Casas.

The flora of the island, taken as a whole, is xerophytic in its tendency. Upon the mountains this tendency manifests itself most strikingly. Not only do plants grow upon the naked rocks, but many plants, such as bromelias, orchids and aroids, grow upon the trees and shrubs without any direct connection with terra firma. Among the trees here were species of *Clusia, Ficus* and *Cecropia,* as well as others not identified. On the rocks mingled with them were *Plumerias, Bilbergias, Fourcroyas* and cacti in great profusion. Palms were abundant, particularly on the perpendicular faces of the mountains, and were kept in constant motion by the sea breeze.

The flora of the mountains is very different from that of the plains, and strangely enough the pines are confined to the plains. To the ecologist the mountains afford a most interesting study, and there also remains much to be done before anything like a satisfactory list of the species can be written. The island is completely surrounded by a mangrove zone.

Here as elsewhere it is the plant that reclaims the sea. The ocean current and tide sweep through it, carrying the débris from other lands, and the roots of the mangrove retain it. It is practically a pure growth, as few other plants can exist under such conditions. It is limited inland by tide-water and is the favorite abode of the *cayman,* many of which may be seen from a ship in passing. Immediately behind the mangrove zone comes a belt of palms, among which are small savannahs in which grasses and sedges form a sward. Nowhere else in the trip were seen such numbers and varieties of palms growing. It reminded one of the palms of the Amazon. Some were palmetto-like, others bore pinnate leaves. Very few were in flower at the time of our visit (January) and the time at our disposal did not warrant our trying to identify them.

Three regions not sharply delimited may be distinguished in the interior of the northern part of the island, the savannahs, the pine lands and the stream banks.

The Malpais River is so named from the wet savannahs in the central part of the island through which it flows. The savannahs also extend to the uplands and have steadily increased in size as the natives have burned them over to improve the pasturage. Besides sedges and grasses there are many other herbaceous plants, especially species of Leguminosæ. They make up a thick sward. All show by their form and the texture and vesture of their leaves a decidedly xerophytic adaptation. Scattered everywhere through the savannahs are arborescent palms mostly of the palmetto type. One species with perfectly rotate leaves and fibrous sheathing bases to their petioles was everywhere seen. Its identity has not yet been determined. The sheaths enclosed one another on the stem, and when separated had the appearance of fibrous cornucopias. Thirty to fifty could be taken from one plant.

The pine lands resemble those of our own gulf region. The pine predominates over considerable areas. They are best developed on the higher ground. They have palms mingled with them everywhere, especially in the lower lands. The kinds of pine have been discussed by the writer in another place. There has been heretofore little done upon the study of their affinities, but in general they have been referred to *Pinus cubensis.* The natives distinguish several kinds and select certain ones for construction. In many of them are large black termites' nests. Not only does the termite infest the island, but ordinary ants are present in large numbers and build large mounds in the savannahs and pine lands. They are a serious obstacle to agricultural pursuits and have, beyond doubt, been an important factor in determining the character of the native vegetation.

Finally along the streams the vegetation shows the least xerophytic tendency, and closely approaches the conditions found in humid tropical regions. Several Scitamineæ occur here, also many ferns and orchids. The trees are mostly broad-leaved and large. Palms abound, also shrubs of many kinds. The soil is rich and very porous. If it were not for the overflow in the rainy season, its agricultural value would be very great.

In conclusion it may be said that the island presents the greatest diversity of conditions. The agriculture of the island, although in a primitive condition, shows this. Tomatoes, potatoes and other crops grown in the north grow well, and at the same time oranges, mammey, guava and all sorts of tropical fruits flourish. It may be truly said that here the vegetations of the temperate and tropical zones meet.

Artificial Sea-water: DR. RODNEY H. TRUE, Department of Agriculture.

Two solutions were tested: (1) A synthetic solution made up with chemically pure chemicals and distilled water, and (2) a solution made by redissolving in a proper volume of distilled water sea salts that had been obtained by carefully evaporating sea water to dryness over a water-bath. The composition of the synthetic solution adopted was the average established by the *Challenger* analyses.

Cladiphora gracilis and *Enteromorpha intestinalis,* together with small scup, silversides and other marine fish and lower animals, were found to live and grow for part of the summer in both solutions. In view of certain unfavorable conditions under which these tests were made, these results make it seem very probable that artificial solutions may be used to replace sea water in some kinds of marine aquarium work.

Notes on the Genus Herpomyces: Professor ROLAND THAXTER, Harvard University.

The morphology and development of *Herpomyces* were described with the aid of diagrams. With the exception of certain species of *Dimeromyces* the genus is the only one among the Laboulbeniaceæ the members of which are parasitic on orthopterous insects, and is of interest from the fact that it adds another to the short list of genera in which the sexual organs are separated on different individuals, which, however, normally develop side by side in pairs corresponding to the spore pairs formed in the ascus. The germinating female spore forms a minute 'primary receptacle,' which gives rise to one or more fertile branches; and the latter, coming in contact with the substratum, form 'secondary receptacles,' which may creep more or less extensively, and, becoming independent of the primary receptacle, after perforating the integument of the host by means of clearly defined short haustoria, produce a variable number of perithecia. The primary receptacle of the male individual is similar to that of the female, and usually produces a variable number of simple antheridia directly; in one instance also producing in addition secondary receptacles as in the female, the perithecia being, however, replaced by tufts of antheridial branches. Although these plants occur on insects (Blattidæ) which are supposed to belong to one of the most ancient types, and are distinctly aberrant when compared with other Laboulbeniaceæ, they do not appear to represent as primitive a type of structure as is found in some other genera, nor do they seem to throw new light on the as yet obscure relationships of the group.

The Contribution of Linnæus and his Students to Phytogeography: DR. HENRY C. COWLES, University of Chicago.

The path-breaking work of Linnæus in taxonomy is well recognized, but phytogeographers have commonly begun their science with Humboldt. As a matter of fact, Linnæus and his students presented a vast amount of material which should be more fully recognized. In his treatise entitled 'Om Växters Planteriog, grundet på Naturen,' published in the first volume of the transactions of the Royal Swedish Academy in 1739, Linnæus outlines a number of the fundamental principles of phytogeography, citing numerous illustrations. Ideas expressed here, as in his various travels and better-known taxonomic works, were worked over in detail by several of his students, and published in the *Amœnitates Academicæ.* Among the best of these treatises were Biberg's 'Œconomia Naturæ' (1749), Tornander's 'Herbationes Upsalienses' (1753), Hedenberg's 'Stationes Plantarum' (1754), and Åmanu's

'Flora Alpina' (1756). Hedenberg's analysis of plant habitats would be a credit to a modern student, and Åmanu's Alpine studies bring out much of value. A trace of the principle of succession of plant associations is found in Biberg, who pictures the changes on a rock surface from lichens to the forest.

Some Notes on the Bending of the Inflorescence of Daucus Carota: Dr. HENRY KRAEMER, Philadelphia College of Pharmacy.

It was observed that the bending of the peduncles of *Daucus Carota* at the close of the day was in inverse proportion to the age of the inflorescence, *i. e.,* this bending is most pronounced in peduncles bearing buds and very young flowers, and decreases with the development of the flowers, so that the oldest flowers show little or no bending of the peduncles. Furthermore, all of these stages were observed on a single plant.

An examination of the anatomy of the peduncles in different stages of the development of the inflorescence showed an increase in the development of mechanical tissues associated with the fibrovascular bundles, the amount of thickening and degree of lignification of the walls of the cells increasing with the age of the peduncles and being greatest in the lower portion and least in the upper part of the same peduncle, and entirely wanting in the peduncles of the buds.

Another observation was that on cool nights after cool days during both summer and fall, when the temperature was about 10° to 15° C., there was a marked diminution in the bending of the peduncles, even in the flower buds, the latter being erect in the majority of cases. On the other hand, this bending was most pronounced in the evening of a hot day when the temperature ranged from 27° to 37° C.

These observations, taken in connection with others, tend to show that the bending of the peduncles of *Daucus Carota* is not due to low temperatures, but that it appears to be influenced by the conditions affecting transpiration and is in the nature of a wilting, this being most pronounced in the young peduncles, which are deficient in mechanical tissues and in which transpiration is most active, and at the close of the day during which the conditions for transpiration have been most favorable.

Studies upon the Cytohydrolytic Enzymes Produced by Soft Rot Bacteria: Professor L. R. JONES, University of Vermont.

The account was based upon studies of *Bacillus carotovorus,* although related organisms were used in comparison. The enzyme was secured apart from the living organism by four methods, as follows: (1) By passing culture broths through porcelain filters, thus removing the organisms and leaving the enzyme in solution in the sterile liquid. (2) By heating broth cultures to 55° C. or slightly above. Since 51° is the thermal death point of this organism, sterility was thus secured, whereas the enzyme, although weakened at 58°, was not fully inhibited until about 62° was reached. (3) By adding the proper amounts of either phenol, thymol or formalin. Chloroform did not sterilize. (4) By precipitation with alcohol.

Detailed studies were made with the enzyme secured by the fourth method including the determination of the following points: (*a*) The relative activity of the enzyme as secured by fractional precipitation with increasing amounts of alcohol; (*b*) the relative activity from filtered as compared with unfiltered broths (porcelain filters); (*c*) the relation of composition of broth, and (*d*) of age of culture to enzyme production; (*e*) minimum, optimum and

maximum temperatures for cytohydrolytic action; (*f*) relative activity in the presence of varying amounts of sodium hydrate and of each of the following acids: hydrochloric, acetic, oxalic, formic, citric, malic, tartaric; (*g*) relative activity in the presence of the juices of the host plants of this bacillus (carrot, tomato, etc.); (*h*) relative activity in the presence of the products of growth of the organism. Practically no diastatic action occurred.

The full paper will soon be published.

A New Key to the Phylogeny of the Monocotyledons: Professor E. C. JEFFREY, Harvard University.

Recent extensive investigations of the anatomy of the higher plants, living and fossil, have established beyond question that anatomical features, especially in the case of the larger groups, are even more constant than those presented by the reproductive and floral organs. This being the case, it is not surprising that they should be used to an increasing extent in the elucidation of phylogeny. The intention of the present abstract is to call attention to the fact that there are certain anatomical features of the Monocotyledons which appear to be of considerable phylogenetic value.

It has long been known that the bundles of the aerial stem of the various monocotyledonous orders are of the closed collateral type, while those of rhizomes often present a curious concentric condition, in which the phloem, exactly reversing the arrangement found in the vascular cryptogams, is surrounded completely by xylem. The latter type of bundle has been called by Strasburger amphivasal, to distinguish it from the amphicribral concentric bundle, which is characteristic of the vascular cryptogams.

The author has found that the amphivasal type of concentric bundle is present not only in monocotyledonous rhizomes, but in the nodal regions of the reproductive axis as well. The amphivasal concentric bundles of the reproductive axis make their appearance at a varying distance below the nodes, and usually disappear entirely from the stem after the leaf-traces have passed off. The reproductive axis is consequently divided into a number of distinct phytomeres, which are characterized at their upper ends by the presence of the amphivasal concentric bundles just described. Sometimes in the extreme upper part of the floral axis, where the internodes become shortened, the amphivasal nodal segments of the axis are fused, so that the fibrovascular tissue becomes continuously concentric, just as is ordinarily the case in monocotyledonous rhizomes.

The occurrence of concentric bundles at the nodes of the reproductive axis has been made out by the author, in the Gramineæ (*Zizania, Phleum, Coix, Zea, Calamogrostis, Elymus,* etc.), Cyperaceæ (*Scirpus, Eriophoron, Cladium, Carex,* etc.), Juncaceæ and certain of the lower Liliaceæ. He believes that these facts furnish a valuable additional clue to the phylogeny of the Monocotyledons.

It is a well-established general principle, resulting from the study of the comparative anatomy of living and fossil gymnosperms, equisetales, etc., that ancestral anatomical conditions are extremely apt to persist in the reproductive axis. The occurrence of concentric bundles at the nodes of the reproductive stem in the above groups is consequently, in all probability, to be regarded as an ancestral feature. This view gains force from the fact that in *Potamogeton,* etc., and many grasses, the concentric bundles occur throughout the stem, but only at the nodes. Moreover, in the higher Liliaceæ, the Iridaceæ, the Orchidaceæ, the aroids, the palms and the Scitamineæ, etc., concentric bundles have

entirely disappeared from the nodes of the reproductive axis.

Briefly, the author's hypothesis is that the primitive monocotyledon was a segmented plant, composed of phytomeres, and characterized by the presence of concentric bundles at the nodes. Probably as the result of periodically recurring unfavorable conditions of existence, the primitive segmented type of stem became differentiated into vegetative and reproductive portions of very different structure. The vegetative part of the stem gradually became characterized by tufted leaves and short internodes, resulting finally in the fusion of the nodal segments, containing concentric bundles to form a continuous system. In the reproductive axis of the lower groups of Monocotyledons, on the other hand, the ancestral division of the stem into distinct phytomeres is retained, together with the recurring segments of concentric bundles. In the higher monocotyledons, however, the primitive organization disappears and concentric bundles are no longer found in the reproductive axis.

The hypothesis outlined above is based on the study of a considerable number of facts, and, further, seems to gain force from two considerations. In the first place, it agrees on the whole very well with the data supplied by a study of the floral organs. Secondly, a typical cambium has been found in the reproductive axis and seedlings of some of the lower monocotyledonous orders mentioned above. The latter feature is reserved for subsequent consideration, but it may be pointed out that this discovery lends support to the opinion recently expressed by Queva, in connection with his anatomical studies on the Uvulariaceæ, viz., that the Monocotyledones are derived from the Dicotyledones, or an equivalent stock, by the loss of a cambium and an increase in the number of leaf-traces.

W. F. Ganong,
Secretary.

SCIENTIFIC BOOKS.

Morphogenetische Studien. Als Beitrag zur Methodologie Zoologischer Forschung. By Tad. Gabrowski. Gustav Fischer. 1903.

Gabrowski publishes under the above title a quarto monograph of which the first 24 pages deal with the structure of *Trichoplax adhærens*, 9 pages with the biology of this animal, and 141 pages of general discussion.

In regard to the structure of *Trichoplax* very little that is essentially new is added. The organism is disc-shaped and, as a rule, irregular in outline. It has an outer layer of ciliated ectoderm, and an internal spongy parenchyma. It lacks completely digestive tract, reproductive organs and nervous system. Some of the parenchyma cells, although not differentiated into muscles, are probably contractile, and cause the changes in the shape of the body.

Trichoplax moves slowly over solid bodies by means of the long cilia on its under surface. No food particles of any sort have ever been found in the body, and the author's only suggestion is that the food may be soluble organic matter absorbed from the surrounding water; but this is purely conjectural, and nothing new was discovered as to the probable source of food.

Reproduction is by division into two pieces; the body drawing away in two directions until the connecting part is finally broken. Gabrowski has also seen two, and even three, individuals come together and fuse into a single mass, for which process he suggests the use of the term concrescence—a term that has acquired a very different meaning, and it seems unfortunate to apply it to this process of fusion.

A long discussion of the affinities of *Trichoplax* leads the author nowhere, since no new facts of any significance have been added by his work and the speculation is not particularly illuminating. Even less impressive is the long, heavy discussion of the gastrula theory which is painfully dragged through

37 pages. All the old, threadbare opinions and speculations that have formed the staple of embryological literature for the last twenty-five years are tediously passed in review—only once more to reject the gastrula theory, a conclusion already reached by so many writers that it would be tiresome merely to cite their names.

The germ-layer definition is 'analyzed,' by which is meant more empty surmising. Finally the reader, if he has not long since lost interest in the protracted discussion, is rewarded by a sort of diversion on 'physiological morphology,' where more commonplace and vacuity are in order.

When morphologists, on the slender basis of a few, new, trivial histological details, can trespass on the time of their fellow-workers to the extent of 174 quarto pages of antiquated discussion, it is, indeed, time to fly from such company and seek new fields where the length of a contribution may be expected to bear some relation to the importance of the discoveries. T. H. M.

Biological Laboratory Methods. By P. H. MELL. Pp. xii + 321. New York, The Macmillan Co. 1902.

It is difficult in a brief statement to do justice to the work of Dr. Mell. We may, however, find the task simplified when we realize that a very considerable amount of the space is devoted to the 127 figures, many of large size, almost all of which are taken from the catalogues of dealers in laboratory and microscopic supplies, and in other apparatus more or less pertinent to the needs of the biologist. Indeed, the addition of an appendix containing a list of prices would have rendered the publication of catalogues by these dealers for some time hereafter a work of gratuity.

For the rest of the book—say sixty per cent.—it may be said to contain a detailed account of a large number of photographic and microscopic apparatus and methods for most of which the beginner in biology—for whom the work is intended as a text-book in a strict sense—will scarcely have use. The same may be said of the very numerous directions for the preparation of tissues. It is remarkable in such a text-book, the rationale of which is to enable the beginner to 'build only the foundation' of biological study, that the for him more simple and useful methods of making simple microscopic preparations of fresh tissues are chiefly omitted. But, of course, we are rapidly passing beyond the pitiable simplicity of ante-microtomic days. The young student of nowadays will, with Dr. Mell's book, get an elaborate knowledge of chromatic aberration and numerical apertures. He will then devote himself to a careful and somewhat exhaustive study of microtomes, following which he will address himself to the numerous special methods of killing, hardening, clearing, imbedding and the like, and of photography, bacteriological methods, injection, maceration and polarization in the order named. The student, having mastered these things, will then presumably be ready for the study of biology in the narrower sense, that, namely, of plants and animals themselves. F. E. LLOYD.

Oeuvres Complètes de J.-C. Galissard de Marignac; Hors-série des Mémoires de la Société de Physique et d'Histoire Naturelle de Genève. Geneva, Ch. Eggiman et Cie; Paris, Masson et Cie, et al. Vol. II. 4to. Pp. 840.

This volume completes the admirably executed reprint of the researches of the great Swiss chemist, the first volume of which was reviewed by SCIENCE on January 16, 1903 (p. 111). The final volume contains Marignac's most important memoirs on atomic weights, a number of interesting and clear-sighted papers concerning various rare elements, several critiques and many papers upon physico-chemical subjects, including his important researches on the specific heats of solutions. At the end is a list of the atomic weights determined by Marignac, in parallel column with the 'International' values of 1903—a comparison which redounds greatly to Marignac's credit. A classified index covering both volumes completes the collection, leaving nothing to be desired. The editor, M. E. Ador, is much to be congratulated on the success of his work.

THEODORE WILLIAM RICHARDS.

SOCIETIES AND ACADEMIES.

AMERICAN MATHEMATICAL SOCIETY.

A regular meeting of the American Mathematical Society was held at Columbia University on Saturday, February 28, 1903, extending through the usual morning and afternoon sessions. Thirty-one members were in attendance; fourteen papers were presented. The president of the society, Professor Thomas S. Fiske, occupied the chair, being relieved at the afternoon session by Vice-President Professor W. F. Osgood. The council announced the election of the following persons to membership in the society: Professor F. H. Bailey, Massachusetts Institute of Technology; Mr. A. T. Bell, High School, Reynolds, Ill.; Professor F. P. Brackett, Pomona College, Claremont, Cal.; Mr. W. E. Breckinridge, Morris High School, New York, N. Y.; Professor Ellen L. Burrell, Wellesley College; Miss E. B. Cowley, Vassar College; Professor E. E. De Cou, University of Oregon; Mr. F. D. Frazer, University of Oregon; Professor J. Willard Gibbs, Yale University; Dr. C. N. Haskins, Massachusetts Institute of Technology; Mr. A. C. Lunn, University of Chicago; Mr. C. L. E. Moore, Cornell University; Mr. F. G. Reynolds, College of the City of New York; Mr. C. E. Stromquist, Yale University; Professor W. E. Taylor, Syracuse University; Mr. Charles Van Orstrand, U. S. Geological Survey. Five applications for admission to the society were received.

Professor E. W. Brown was reelected a member of the editorial board of the *Transactions* for a term of three years. The office of assistant secretary of the society, vacated by the appointment of Dr. Edward Kasner to the editorial staff of the *Transactions,* was abolished.

It was decided to hold the summer meeting of the society at Massachusetts Institute of Technology during the week beginning August 31. A colloquium will this year be held in connection with the summer meeting. Courses of three to six lectures will be given as follows: By Professor E. B. Van Vleck, 'Selected topics in the theory of continued fractions and divergent series'; by Professor F. S. Woods, 'The connectivity of non-euclidean space'; by Professor H. S. White, subject to be announced.

The following papers were read at the February meeting:

L. P. Eisenhart: 'Congruences of conics.'

Emory McClintock: 'The logarithm as a direct function.'

H. P. Manning: 'Non-euclidean geometry of nets of circles.'

C. E. Stromquist: 'A generalization of the length integral.'

Edward Kasner: 'Three notes on projective geometry.'

W. B. Ford: 'A theorem concerning the functions of two or more complex variables.'

W. F. Osgood: 'The integral as the limit of a sum, and a theorem of Duhamel.'

E. R. Hedrick: 'The integral curves of a partial differential equation.'

E. B. Van Vleck: 'On an extension of the 1894 memoir of Stieltjes.'

A. S. Gale: 'On a generalization of a set of associated minimum surfaces.'

G. A. Miller: 'A fundamental theorem with respect to transitive substitution groups.'

E. W. Brown: 'On the derivatives of the lunar coordinates with respect to the elements.'

Charlotte A. Scott: 'On the fundamental theorem of projective geometry.'

Alfred Loewy: 'Ueber die Reducibilität der reelen Gruppen linearer homogener Substitutionen.'

After the meeting many of the members present dined and passed the evening together.

The next meeting of the society will be held in New York on April 25. The Chicago Section will meet at Northwestern University, Evanston, Ill., on April 11. The San Francisco Section will hold a meeting early in May.

F. N. Cole,
Secretary.

THE SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE.

Several New York biologists met at the home of Professor Graham Lusk, on January 19, 1903, to consider the advisability of organizing a society for experimental biology and medicine. This project was originally suggested by Dr. S. J. Meltzer. Those present at this meeting were unanimously in favor

of the plan suggested. The temporary chairman, Professor Frederic S. Lee, appointed Drs. Meltzer, Lusk and Gies a committee on constitution and by-laws.

On the 25th of February the meeting for permanent organization was held in the Laboratory of Physiological Chemistry of Columbia University. A constitution was adopted, officers were elected and a program of experimental demonstrations was successfully carried out.

The objects of 'The Society for Experimental Biology and Medicine' are, as indicated in the constitution, 'the cultivation of the experimental method of investigation in the sciences of animal biology and medicine.' "Any person who has accomplished a meritorious original investigation in biology or medicine by the experimental method shall be eligible to membership." "Every member shall be expected to conduct an experimental investigation, and give public notice of it, at least once in two years. Non-compliance with this requirement carries with it forfeiture of membership." "The program of each meeting shall consist in brief presentations of the essential points of experimental investigations in biology and medicine or allied natural sciences, preferably of *demonstrations* of actual experiments." The meetings will be held in suitable laboratories.

The officers elected to serve for the ensuing term are:

President—Dr. S. J. Meltzer.
Vice-President—Dr. Wm. H. Park.
Secretary—Dr. William J. Gies.
Librarian—Dr. Graham Lusk.
Treasurer—Dr. Gary N. Calkins.

The following demonstrations were made:

An experiment to show the difference in effect between the simple cutting of the cervical sympathetic and the removal of the superior ganglion: S. J. MELTZER.

Dr. Meltzer presented a rabbit in which the cervical sympathetic had been cut on one side, and the superior ganglion had been removed on the other side. Both pupils were of the same size. About two hours before the demonstration one hind leg was tightly constricted and 1 c.c. adrenalin injected into it (peripheral to the ligature). On removal of the ligature the pupil on the side from which the ganglion had been excised became greatly dilated, while the pupil on the other side remained unaffected.

Differentiation of monkey blood from human blood by the precipitin serum test: JAMES EWING.

The serum used by Dr. Ewing in this demonstration was obtained from a chicken which had received five injections each of 10 c.c. of human placental blood. This serum proved to be much more selective than the ordinary humanized rabbit serum. The chicken serum in various dilutions up to 1–100 was added to specimens of human and monkey serum in dilutions also of 1–100. It produced turbidities in all the specimens of human blood, but failed entirely to affect the monkey blood. Finally, the chicken serum was added in a dilution 1–5 to specimens of both human and monkey blood. In the human blood a milky ring formed instantly at the line of junction of the test serum with the human serum, and a flocculent precipitate formed in fifteen minutes, while in the monkey serum no change whatever could be observed.

An improved cage for metabolism experiments: WILLIAM J. GIES.

A cage specially designed for experiments on dogs was shown. The parts are so adjusted as to favor the collection and separation of feces, urine and hair. The improvements consist mainly of mechanical devices suggested by experimental experiences of the past few years in metabolism work, all of which are designed to insure quantitative accuracy as well as comparative convenience in the collection of excreta.

Properties of 'Bence Jones's body': WILLIAM J. GIES.

Through the kindness of Dr. Meltzer a patient's urine containing this substance had been placed at our disposal for chemical study. Some of the results of this investigation were presented and various properties of the body demonstrated. Special attention was drawn to a test of Boston's new method of detecting 'Bence Jones's body' in the urine.

WILLIAM J. GIES,
Secretary.

NEW YORK ACADEMY OF SCIENCES. SECTION OF ANTHROPOLOGY AND PSYCHOLOGY.

The regular meeting of the section was held January 26, in conjunction with the American Ethnological Association, Professor Thorndike presiding. The first paper was presented by Dr. Maurice Fishberg, 'The Ancient Semites and the Modern Jews.' The somatic characteristics of the ancient and the modern Semites were discussed in detail, the purest representatives of the latter being the Arabian Bedouins. Their anthropological type is distinctly African. The bas-reliefs of the ancient Semites, as represented on the Assyrian and Egyptian monuments, are of the same type. The modern Jews are, on the other hand, a distinctly Asiatic type physically; they are brachycephalic—cephalic index 82 with less than five per cent. of heads having an index of 75 or less. Their head form shows very little variability, but one important feature is that in countries where the non-Jewish population is round-headed the Jews are also round-headed. In Caucasia their cephalic index is 87; in eastern Europe, where the cephalic index of the non-Jews ranges between 80 and 84, that of the Jews is about the same. In Africa, among the long-headed Gentile population, the Jews are also dolichocephalic. The same is observed to be the case with stature. The Jews are taller in countries where the general population is tall. The type of the Jew is dark, but 12 per cent. of pure-blood types, having fair hair and blue eyes, are to be found. The nose of the modern Jew is not as frequently hooked as is generally supposed. Statistics show that only 12 per cent. are of this variety. The only characteristic which often betrays a Jew is the 'Ghetto eye.' But such Jews who have lived outside of the pale of the Ghetto for a few generations do not present this phenomenon. Physically there are two types of Jews—one derived from Asia, commonly called *Ashkenasim,* and constituting more than 90 per cent. of the modern Jewry. It has no relation at all with the second type, of African origin, commonly referred to as *Sephardim.* These, constituting less than 10 per cent. of the Jews, alone are more or less related to the ancient Semites, although they have not everywhere preserved themselves as pure as in Africa. Besides these there are to be discerned other subtypes, in which Teutonic, Slavonic and Mongolian blood appears most prominent. From the standpoint of physical anthropology, the view that all the modern Jews are descendants of Abraham, Isaac and Jacob, can not be seriously considered. The only thing which binds the modern Jews together is their religion. In blood there is no more relation between the Jews than there is between the people who profess the protestant, methodist or unitarian religion.

Mr. H. H. St. Clair, 2d, then read a paper, 'Investigations among the Comanche and Ute Indians.' The investigations were made during the summer of 1902 upon the Comanches on the Kiowa-Comanche Reservation, Oklahoma, and the Utes of the Uintah Reservation, Utah. Both tribes belong to the great Shoshonean family. These tribes have a very loose social organization and no elaborate religious ceremonial. There are no calendar-records nor any traces of heraldry among the Comanches. The designs painted on rawhide bags or woven in beads have no meaning as with the Shoshones, but are merely ornamental, and there is lack of the symbolic conversationalism found among such people as the Arapahoes and Sioux. In their stories the coyote figures as the most frequent character representing the fool and schemer. There are striking similarities between the Shoshone and Nahuatl languages of Mexico, each using the same grammatical processes in its pronoun, noun, preposition and verb, and the order of words and structure of the sentence being practically the same in both.

James E. Lough,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THERMODYNAMICS OF HEAT-ENGINES.

To the Editor of Science: In undertaking to express to you, and through your columns to Dr. Thurston, my appreciation of his very generous review of my 'Thermodynamics of Heat-engines,' will you allow me to call at-

tention to one fundamental point in my position which the latter failed to grasp upon first reading.

Dr. Thurston quotes me in the following words: "The much-discussed 'Second Law of Thermodynamics' takes the form: 'The entropy of the world tends to a maximum and the temperature to a minimum.' It is, however, pointed out, etc."

These words are correctly quoted (page 35), but their significance has been directly reversed by omission of the context. The statement of the second law just quoted is given by me as itself a quotation of its heretofore accepted form, for direct contrast with my own statement of it, which will be found (on pages 25 and 35, with elaboration and explanation in the intervening pages) in words which may be condensed into the following, for present purposes:

"That while any given quantity of energy tends, so long as it exists without transformation, to fall in intensity, and never the reverse, *yet* the secondary form of energy into which that quantity may at any time find itself transformed possesses a degree of intensity which is *entirely independent of that of the original quantity,* and which is *the maximum permitted by circumstance.* In other words, energy tends downward in intensity during untransformed existence and upward during transformation."

This necessarily denies *in toto* the doctrine of the dissipation of energy. It affirms, on the contrary, that as much exaltation of energy is constantly going on as there is of depression of energy. In short, the total fund of intensity or availability of the energy of the universe is as constant as is the universe's total fund of mass, or as is its total fund of the product of the two, energy itself.

The availability of the energy of the solar system is, of course, being steadily dissipated. But progress in astronomy has generations ago passed the point when observations confined to the solar system suffice for the establishment of fundamental principles such as these.

The new statement of the second law takes on especial importance as being, if true, one link in the chain of evidence confirming the unity of the universe, the modern idea of which was so interestingly referred to recently by Professor Newcomb. The doctrine of the dissipation of energy necessarily excluded any possibility either of the universe being infinite and eternal in its extent or of its being one with the solar system. The new statement not only is consistent with those ideas, but it is implied by or implies them, whichever end of the sequence the thinker may prefer to regard as the natural origin.

SIDNEY A. REEVE.

THE JUDITH RIVER BEDS.

THE reader of Professor Osborn's recent note in SCIENCE on the 'Age of the Typical Judith River Beds' would be led to infer that I had either denied or questioned the Upper Cretaceous age of these beds. Since this note places me in an entirely false position on this question, I wish to offer the following brief remarks by way of explanation.

1. I have never even so much as questioned the Upper Cretaceous age of the Judith River beds. The point I raised was as to their stratigraphic position within the Upper Cretaceous relative to the Pierre.

2. Osborn's statement that since Cope, Cross, White and Dana have referred these beds to the Upper Cretaceous, they therefore overlie the Pierre is unwarranted, since these authorities and American geologists generally have heretofore included everything from the Dakota to the Laramie in the Upper Cretaceous. Would Professor Osborn place the Dakota, Benton and Niobrara above the Pierre because those same authorities have referred these deposits to the Upper Cretaceous?

3. All who are familiar with the literature on this subject know that the Judith River beds have been referred to different ages by Hayden, Meek, Leidy, Cope, Marsh, White, Stanton, Cross, Lesquereux, Newberry and others, varying from Lower Tertiary on the one hand, to Lower Cretaceous or Upper Jurassic (Wealden) on the other, and that, therefore, Osborn has *not* 'abundant authority for the statement that among geologists of the United States there has never been any ques-

tion as to the Laramie or Upper Cretaceous age of the typical Judith River Beds.'

4. Since Hayden's stratigraphical observations near the mouth of Little Rocky Mountain Creek do not harmonize with the paleontological correlations of Drs. White and Stanton at the mouth of the Judith River, and since no one has ever revisited the first locality and reversed Hayden's determinations by a reexamination of the stratigraphy, I believe the exact stratigraphic position of the Judith River beds remains unsettled and that it is premature to assert that 'the true Judith River beds certainly overlie the Ft. Pierre and are of more recent age,' although this is now very generally believed and may eventually prove to be the case.

J. B. HATCHER.

BOTANICAL NOTES.

VEGETABLE GALLS.

THESE curious growths, which result from the action of two organisms, have not received the attention of botanists which they deserve. That they develop because of the presence of some insect, or as a consequence of the sting or puncture of another insect, does not make them less vegetable in nature. A prickly gall on a rose leaf is a rose structure as truly as the rose fruit is, and its growth and development are as properly the objects of study by the botanist as are the growth and development of any other plant structures.

Mr. Edward Connold, an English botanist, has recently brought out a most interesting book on 'Vegetable Galls,' which must help to direct the attention of botanists to this neglected field. By means of fine half-tone reproductions of photographs he shows more than one hundred galls and their variations, and to these he has added descriptions which bring out quite methodically their structural characteristics, and their relation to the causal parasites. In treating the subject the author groups galls into: (1) Root galls, (2) stem galls, (3) leaf galls and (4) flower and fruit galls. Of the first he illustrates six kinds by as many plates. Among the thirty-one plates of stem galls perhaps the most suggestive are numbers 23 and 24, which show galls on the twigs and stems of *Salix cinerea* caused by the larvæ of *Agromyza schineri*, and which so closely resemble the early stages of the 'diamonds' on the 'diamond willow' of the Great Plains as to suggest similarity of origin. Of leaf galls there are no less than sixty-three plates, representing a great number of different forms much like those found on leaves in this country. Twelve plates are given to the illustration of the galls on flowers and fruits, including two in which the galls are the familiar 'plum pockets' due to the presence of the minute fungus *Exoascus insititiæ*.

A similar work should be undertaken in this country. Mr. Connold has set a good example, showing us how to illustrate as well as how to treat the subject. No doubt the text is capable of improvement, and yet we should not object to a work in which the text was patterned directly after that found in the English book. Here is an open field for some of our active young botanists to enter.

POPULARIZING THE STUDY OF FUNGI.

ANY book which increases popular interest in any department of botany should be welcomed by scientific men, even though the treatment may not be quite like that in works designed to be used by students and professors in the colleges and universities. No doubt those of us who belong to the latter class are quite too much inclined to measure the value of every book by our own needs and standards. We commend the book which meets our wants and which is so written that it seems to be addressed to us or our students, and too often we deem of little value the book in which we find nothing new for ourselves, although it may appeal directly to many other people who know less about the subject. That there are some popular books which are simply atrocious is true, and the present writer has been obliged to denounce them in strong terms, and yet it is an open question whether even the worst of these are wholly bad. With their crude drawings and barbaric coloring, they may appeal to certain classes of untrained minds much more than the ele-

gantly drawn figures to be found in some of our best works. We must not forget that wood-cuts precede steel plates, that 'chromos' are antecedent to the appreciation of good oils and water colors, and that gaudy adornment is the forerunner of that finer and nicer ornamentation that prefers quietness of form and color.

All this is apropos of a book on the fungi—really on the toadstools and mushrooms—prepared by an enthusiastic amateur fungologist, Captain Charles McIlvaine, with the title 'One Thousand American Fungi.' We are told that a score of years ago, while the author was living in the mountains of West Virginia, he became interested in the luxuriant growths of fungi which he saw in his rides through the dense forests. Beginning with a gastronomic interest (which in fact still dominates his work), he has widened his field of interest so as to take in much of what we are pleased to regard as scientific. Gradually the idea of preparing a book took form, and the result is a large octavo volume of more than seven hundred pages, and including a couple of hundred illustrations, many being colored plates or half-tone reproductions of excellent photographs. In order to secure the information he desired in regard to the edible qualities of fungi, he had personally to test "hundreds of species about which mycologists have either written nothing or have followed one another in giving erroneous information." He naïvely refers to the frequent 'unpleasant results' following such personal tests, but in the end he felt repaid by "the discovery of many delicacies among the more than seven hundred edible varieties" which he found. Such work constitutes real investigation. It is laboratory work of a special kind, but while its purpose is the discovery of gastronomic facts, they must be included in the mass of knowledge and experience which we call science. While appealing primarily to the mycophagist, this book will be found useful to the mycologist also.

MARINE LABORATORY BOTANY FOR 1903.

THE annual announcements for the season of 1903 of three water-side laboratories are at hand. The first of these is that of the Marine Biological Laboratory at Woods Holl, Mass. Here, as in former years, the botanical work will be under the general direction of Professor Bradley Moore Davis, of the University of Chicago. The work may be under supervision or without supervision. Under the former, courses are offered in morphology, physiology, cytology, ecology, and the morphology and taxonomy of the thallophytes. For these the usual fees are charged. Investigators who wish to take up lines of work without supervision may be accorded the privilege free of expense by making application to Dr. Davis and complying with certain requirements. The session begins July 1 and ends August 12.

The Minnesota Seaside Station, at Port Renfrew, on Vancouver's Island, will open about the middle of July and close about the first of September. As in former years, the station is to be under the direction of Professor Conway MacMillan, of the University of Minnesota, Minneapolis. The party is to leave Minneapolis *viâ* the Canadian Pacific Railway 'about July 15,' and return to Minneapolis 'about September 1, making two stops, one at Glacier, the other at Laggan.' "Classes in elementary and advanced botany will be formed for high-school teachers and undergraduate college students." Advanced workers will find many problems awaiting their independent investigation.

The Ohio State University Lake Laboratory will be open again at Sandusky, and, as heretofore, will include work in several lines of botany. This year there are offered general botany (the study of type forms, from the lowest to the highest orders), ecology, systematic botany, and the morphology and taxonomy of algæ and fungi. As the work is all under Professor Kellerman, this is a sufficient guarantee of its high quality. Instruction begins June 29 and closes August 7, but the laboratory does not close until somewhat later in the summer.

CHARLES E. BESSEY.
THE UNIVERSITY OF NEBRASKA.

ITHACA, N. Y., WATER-SUPPLIES.

A 'Committee of Ten' appointed by the Business Men's Association of Ithaca and other prominent citizens has been engaged, since the epidemic of typhoid appeared in that city and in anticipation of the taking over of the water-works system by the municipality, in exploring for artesian supplies. There are many flowing wells in the district eastward of Ithaca, especially at Freeville, some ten miles away, and on the upper levels of that section, near the city. In the city of Ithaca are two such wells, the one, which has been flowing for a number of years and, as stated by the proprietor, with increasing volume, was found when measured by the committee to deliver 403,000 gallons per day. The other is supplying the Ithaca Salt Works with water at the rate of between 600,000 and 700,000 gallons. The committee has bored one new, a flowing well, and is now engaged in boring others to depths of from about two hundred to three hundred feet, reaching strata of water-bearing gravels overlaid with heavy beds of clay and with water under pressures of considerable magnitude. The members of the committee state that there is no question that an absolutely pure, germ-free water may be obtained in ample amount to supply the city and with a large surplus.

The analysis of the water, as given by Chamot, is as follows:

	Parts per million
Free ammonia	0.480
Albuminoid ammonia	0.005
Nitrogen as nitrites	none
Nitrogen as nitrates	trace
Oxygen consumed	0.644
Chlorine as chlorides	61.640
Total solid residue	304.000
Loss of solids on ignition	78.000

"The different portions taken gave from 0 to 13 colonies of bacteria per cubic centimeter. No objectionable species were detected.

"From the chemical analysis it is be concluded that the water is free from any present contamination and is therefore a good, safe drinking water; while from the bacteriological standpoint it would be considered a well of exceptional purity."

The committee includes Professor R. S. Tarr, professor of geology, Cornell University, who has long been familiar as a specialist with the geology of the region, Mr. Edgar Kay, of the College of Civil Engineering, an expert in hydraulic and water-works engineering, Mr. M. E. Calkins, president of the Cayuga Lake Cement Co. and the Ithaca Salt Works, a business man of extensive experience in the exploration of the salt beds of the state and an expert in matters relating to deep wells and the location of water-bearing deposits, Mr. R. H. Thurston, director of Sibley College, is consulting member relative to machinery, Mr. F. M. Rites, formerly of the Westinghouse Company, an experienced mechanic, inventor and designer, and several business men more or less familiar with the conditions determining the location of deep wells at Ithaca and its neighborhood. Two are members of the City Council. The chairman of the committee is Judge Almy, now Surrogate, and the treasurer is Mr. C. D. Bouton, ex-mayor of Ithaca.

It is the plan of the committee to ascertain precisely what are the possibilities and the practicable ways of securing for the city of Ithaca such water as is above referred to. The people of Ithaca are practically unanimous in their determination to secure such a supply if possible. The indications thus far are thought to be that a gravity supply may be had from the high lands east of Ithaca or that now well-known and probably unlimited artesian supplies beneath the city itself may be availed of by pumping. Very possibly the latter may be taken as a temporary resource while exploiting the Freeville district for a permanent gravity system. Meantime, filtration will serve, if delay occurs, until pure, clear, soft and germ-free artesian water is thus obtained.

Throughout the late epidemic, an ample supply of this water has been had and freely used, with other fine spring waters. The University has supplied this water on the campus and, where called for, to students.

R. H. Thurston.

March 12, 1903.

PRESENTATION OF A BUST TO PROFESSOR CHAMBERLIN.

PROFESSOR J. C. BRANNER, of Stanford University, proposed at the meeting of the American Association for the Advancement of Science, in 1901, to present a bust of Professor T. C. Chamberlin to the University of Chicago in recognition of his eminent services to the science of geology. A number of other geologists joined Professor Branner, and the bust was presented with appropriate ceremonies on February 7. The principal address was made by Professor Van Hise, who gave an account of Professor Chamberlin's investigations, in the course of which he said:

"Professor Chamberlin has the speculative power of the Greeks in seeing lines along which a solution may lie; but, unlike the Greeks, is not content when a possible solution has been suggested. After the modern scientific man has devised various possible solutions he has before him the far more difficult task of determining the *true* solution. The profound difference between the ancient speculative philosopher about science, and the modern scientific man, is that the one requires only a brilliant constructive intellect and reasoning power; while the other requires with this a capacity for patient, laborious, consecutive, constructive work running through years, exhaustive collection of material, observational work in the field, experimental work in the laboratory, verification and reverification, sifting, testing, judging, and thus finding out, not what *may be* the truth, but what *is* the truth. It seems to me that Professor Chamberlin's eminent success as a scientist lies in this two-fold power. With speculative ability only, a man is untrustworthy and erratic. With the power of steady drudgery only, he is mediocre. Combine the two, and he is a scientist of the first rank."

Addresses were also made by President Harper, Professor Salisbury and Dr. Bain, and Professor Chamberlin responded as follows:

"It is quite impossible for me to express in any fitting way the feelings that arise in response to this very unusual honor. I was surprised when the request—put in the jocular form of command—to sit to Mr. Taft, came to me. I have not ceased to be surprised ever since, and I am more surprised to-day at the terms that have been used in this presentation. If there have been two things that have been supreme objects of aspiration to me on the professional side, they are the desire to develop and present some truths that shall live as long as man shall have need of truths; and the other, that I may touch by some small measure of inspiration young minds with longer lives and with better preparation for the work of the future than are granted to me. My students and my colleagues know that as a result of my studies I make no limited estimate or forecast of the future of the earth and of its possibilities, of the future of man and his great development. I see no early and final winter; I see no portending calamity to this earth. I see a possibility, a probability, almost a certainty, of millions of years of human endurance on the earth; and, in view of that fact, when I recognize that every truth lives and works every day and every hour, by night and by day, I feel that, even though a small truth be brought forth and sent upon its mission, in the long ages in which it has to work it can not but do great things. And when I think of the influences which young men and young women, coming to the active spheres of life with greater advantages than those of us of the past have had, will exert in the fulness of time; when I realize that they will be able to transmit to others, and these to others, and to others still, the measure of thought that comes to them—though I realize that all this must lose its personal relationship to its author and must be submerged in the common flood of influences that will commingle with it as time goes on—yet, it is a pleasant and inspiring thought that these, too, shall work and that the truth sown shall be fruitful as long as man walks upon the earth. It is especially grateful to me to hear to-day from my colleagues in science, from those whose judgment I must respect, such expressions regarding the scientific investigations which I have been permitted to make. It is also especially gratifying to hear the expressions of

appreciation of those whom I have been privileged to lead in the early paths of truth. I can not express all that I would. I hope that you will take my wish in place of my inability."

THE SMITHSONIAN INSTITUTION.

The board of regents of the Smithsonian Institution held an adjourned meeting on the morning of March 12, all the members being present with the exception of Senator Cullom, President Angell, Mr. Olney and Dr. White.

The chancellor, the chief justice of the United States, reported on behalf of the committee appointed at the last meeting of the board, to consider the whole subject of defining the powers and duties of the executive committee. Two meetings of the committee had been held, but two members, Senator Cullom and Representative Dinsmore, had been unable to attend, and the other members of the committee, considering the importance of the subject entrusted to their consideration, would not take the responsibility of making a report unless the matter could be considered by the full committee. The chief justice expressed the opinion, however, that the committee realized that under present arrangements too little time was afforded the regents for the consideration and discussion of the important matters entrusted to their care. He thought that there should be more frequent meetings of the board of regents, and regular and stated meetings of the executive committee. Senator Platt and Representative Adams of the committee agreed with him in this, and Representative Adams offered a resolution providing for three meetings of the board of regents each year: One, the annual meeting in January, for the transaction of the usual routine business, and the others—one on the sixth of December, and one on the Tuesday following the first Monday in March—for the discussion of the affairs of the institution, and for a free interchange of views among the members. This resolution was passed unanimously.

In the discussion the opinion was very generally expressed that the executive committee also should hold more frequent meetings, and that they should have regular and stated meetings for the discussion of the affairs of the institution, but the members thought that the executive committee should provide for its own meetings, and that this was not a matter calling for the action of the board.

The members also very generally expressed the opinion that the board was not ready to define the powers and duties of the executive committee—that this demanded careful consideration and an examination of the organization, and of the United States statutes referring to it. It was therefore moved that the committee be continued and that it should make a report upon the subject at the next meeting of the board which will be on December 6, unless this should fall upon a Sunday, in which case the meeting will be on Monday following. This resolution was passed by the board, and it is understood that in accordance with the suggestion of Judge Gray the secretary will prepare for the use of the board, a pamphlet containing references to all the United States statutes referring to the institution and its allied bureaus.

The subject of the new building for the National Museum came up for consideration. Congress has appropriated the sum of three millions and a half dollars for a new building for the National Museum, and the making of contracts, etc., for the erection of the building has been placed by congress in the hands of Mr. Bernard R. Green. Action was taken looking to the beginning of immediate work upon the new museum building, authorizing the secretary with the advice and consent of the chancellor, and the chairman of the executive committee, to arrange with Mr. Bernard R. Green in reference to carrying out the act of congress.

The question of the management of the government bureaus in charge of the Smithsonian Institution and the policy of the institution towards these bureaus then came up for discussion. Dr. Bell recommended a return to the policy of the first secretary, Professor Henry, and urged the importance of granting autonomy to each bureau. He stated that it was the duty of the regents to consider

carefully, how the usefulness and value of the Smithsonian Institution and its allied bureaus could be improved, and offered the following resolutions:

The secretary shall nominate, and by and with the advice and consent of the board of regents, shall appoint the heads of the various bureaus supported by Congress under the direction of the Smithsonian Institution—*to wit*—the National Museum, the Bureau of American Ethnology, the National Zoological Park, the Bureau of International Exchanges, and the Astrophysical Observatory.

The secretary shall have power to fill up all vacancies that may happen in these offices during the intervals between meetings of the board, by granting commissions which shall expire at the next meeting of the board of regents.

The head of each bureau shall nominate, and by and with the advice and consent of the secretary, shall appoint the subordinates in the bureau under his charge.

The heads of the bureaus shall be termed directors; and the board of regents hereby creates the offices of director of the National Museum, director of the Bureau of American Ethnology, director of the National Zoological Park, director of the Bureau of International Exchanges and director of the Astrophysical Observatory, and instructs the secretary to fill these offices by temporary appointment to expire at the next meeting of the board, when nominations shall be presented for confirmation by the board.

There was no time for adequate discussion of these resolutions and it was believed by all the members that the subject was of too great importance to be passed upon at once by the board. Judge Gray thought that the resolutions should be examined and reported upon by a committee, before asking the board for a decision, and suggested that they might be referred to the committee having under consideration the definition of the powers and duties of the executive committee, for a report. Dr. Bell thereupon withdrew his motion, and moved to refer the resolutions to the committee as suggested by Judge Gray, and this motion was adopted by the board.

The question of the disposition of the remains of James Smithson, the founder of the Smithsonian Institution, then came up for consideration. It will be remembered that the regents had been notified that the body of James Smithson would have to be removed from his grave, in order to make room for a quarry, and that the regents had decided that the remains should be transferred from the cemetery in Genoa, Italy, where they now rest, to another cemetery in the same city. Dr. Bell offered to have the remains removed to this country at his expense, if the regents would take charge of them upon their arrival, and in view of this proposition he moved a reconsideration of the decision of the board relating to the disposition of the body. The regents seemed to be very favorably impressed with the proposition, and in view of the fact that there was no immediate necessity for the removal of the grave, and that no time remained for discussion of the matter, the resolution was allowed to lie over to be acted upon at the next meeting of the board in December. The meeting then adjourned.

SCIENTIFIC NOTES AND NEWS.

THE National Academy of Sciences will hold its annual stated meeting at Washington beginning on Tuesday, April 17.

THE American Philosophical Society will hold at Philadelphia a general meeting on April 2, 3 and 4. The preliminary program contains the titles of thirty-one papers, including one by President Daniel C. Gilman, on 'The Carnegie Institution during the first year of its development,' and one by Dr. W. H. Welch on 'The objects and aims of the Rockefeller Institute for Medical Research.' The sessions will be held in the hall of the society beginning in the morning at 10:30 and in the afternoon at 2. Luncheon will be served to members on each day; there will be a reception to members and ladies accompanying them on Thursday evening, and visiting members will be the guests of resident members on Friday evening.

PRESIDENT ROOSEVELT has appointed the following as a commission to report to him on the organization, needs, and present condition of government work, with a view to including under the Department of Commerce bureaus not assigned to that department by congress:

Charles D. Walcott, Department of the Interior; Brigadier-General William Crozier, War Department; Rear-Admiral Francis T. Bowles, Navy Department; Gifford Pinchot, Department of Agriculture; James R. Garfield, Department of Commerce and Labor.

The Carnegie Institution, on the recommendation of the advisory committee on geophysics, has appropriated $6,000 to be expended under the direction of Dr. T. C. Chamberlin, of the University of Chicago, in research relative to fundamental problems in geology. The investigation will consist of a joint mathematical, astronomical, physical, chemical and geological inquiry into certain phases of the earth problems that lie in the common domain of these sciences. Dr. F. R. Moulton, of the department of astronomy of the University of Chicago; Professor C. S. Slichter, of the department of mathematics of the University of Wisconsin; Professor L. M. Hoskins, of the engineering department of Leland Stanford University; Professor Julius Stieglitz, of the department of chemistry, and Mr. Lunn, of the department of mathematics of the University of Chicago, will participate in the inquiry.

The French Academy of Moral and Political Sciences has elected Professor E. Caird, master of Balliol College, Oxford, a corresponding member of the philosophic section.

M. Bigourdan, astronomer at the Paris Observatory, has been appointed a member of the Bureau of Longitude in the room of the late M. Faye.

President Roosevelt has appointed a board of visitors to the Naval Academy for the coming year as follows: Dr. Henry S. Pritchett, Massachusetts Institute of Technology; Professor H. C. Ellis, of Texas; Mr. Lewis Nixon, of New York; Rear-Admiral George Brown, U.S.N., retired, of Indiana; Captain A. T. Mahan, U.S.N., retired, of New York; Lieutenant R. M. Thompson, U.S.N., retired, of New Jersey; and Mr. John R. Procter, of Kentucky, civil service commissioner.

The Carnegie Institution has made an appropriation to Dr. J. E. Duerden to assist him in his investigations on the morphology of recent and fossil corals. The studies were commenced while Dr. Duerden was curator of the museum, Jamaica, B. W. I., and have been continued at the Johns Hopkins University and the American Museum of Natural History, New York. The principal results thus far are contained in a series of four papers published in the *Annals and Magazine of Natural History*, and in a *Memoir* of the National Academy of Sciences just issued.

Dr. Alexander Graham Bell entertained the board of managers of the National Geographic Society, of which he is president, at dinner on the evening of March 14. It is reported that Mr. Ziegler has invited the National Geographic Society to send a representative without cost to the society on the Arctic expedition that he is planning.

Professor William Beebe, of the mathematical department of Yale University, is at present in Italy.

Professor Barula, the zoologist of the Baron Toll expedition, who left the expedition's yacht *Saria* in May with three others to engage in scientific research in New Siberia, has arrived at Irkutsk, eastern Siberia.

Professor R. H. Thurston, director of Sibley College, Cornell University, gave a lecture before the New York Electrical Society on March 18, the subject being 'The Steam-turbine to date.'

Congress has passed a bill appropriating $125 per month during her lifetime as a pension to Mrs. Emily Lawrence Reed, widow of the late Major Walter Reed, U.S.A., whose important investigations on yellow fever at Havana are well known.

The death is announced of M. Alexis Rousset, the explorer, at Cape Lopez, in the Gulf of Guinea. He was returning from an expedition in the Shari region, where he had discovered and mapped a shorter route through the Tafa region, between Lake Chad and the Congo basin.

Stanford University has secured the library of the late Mr. Konrad, chief hydraulic engineer of the Netherlands.

THE German emperor has approved of a plan for founding an institute for advanced medical education in Berlin as a memorial to the late Empress Frederick.

SENATOR WM. A. CLARK, of Montana, has contributed $250 for the furtherance of the investigations being carried on by the University of Montana Biological Station at Flathead Lake, under the direction of Professor Morton J. Elrod. This is his fifth contribution for this purpose.

THE consul-general for Mexico in Liverpool has received official notification that the Mexican government proposes to give an annual grant of money to the Liverpool School of Tropical Medicine, in whose operations from its formation they have taken a deep interest.

THE London Epidemiological Society held a meeting on February 25 for the discussion of the possible spread of yellow fever to Asia by way of the Panama canal. The discussion was opened by Dr. Patrick Manson, medical adviser to the Colonial Office. A committee was appointed to cooperate with American societies in drawing the attention of the governments of Great Britain and the United States to the question.

PROFESSOR RUSSELL H. CHITTENDEN, director of the Sheffield Scientific School of Yale University, has arranged the Thirty-Seventh Annual Course of Sheffield Lectures, which are now being delivered on Friday evenings, at 8 P.M. Following is the list of lectures, with their subjects:

'Mont Pelée and the Tragedy of Martinique': Professor ANGELO HEILPRIN, of the Academy of Sciences, Philadelphia.

'Storms and Weather Phenomena': Professor WILLIS L. MOORE, Chief of the U. S. Weather Bureau, Washington.

'Peary's Progress to the Pole': Mr. HERBERT L. BRIDGMAN, of Brooklyn, N. Y.

'Our Isthmian Canal': General HENRY L. ABBOTT, of the U. S. Army, Retired, Cambridge.

'Household Art in Japan': Professor EDWARD S. MORSE, of Salem.

'Recent Astronomical Photography': Mr. GEORGE W. RITCHEY, of Chicago University and the Yerkes Observatory.

'Modern Methods and Results of Exploration for Dinosaurs': Professor HENRY F. OSBORN, of Columbia University.

'The Discovery of the Use of the Arteries; or Experiment vs. Subtlety in Biology': Professor JOHN G. CURTIS, of Columbia University.

'The Medicine-Man': Professor ALBERT G. KELLER, of Yale University.

'Professional Codes of Ethics': Professor ROSSITER W. RAYMOND, Secretary of the American Institute of Mining Engineers.

'The Land of Ophir': Professor JOHN HAYS HAMMOND, of the Sheffield Scientific School.

THE following cablegram has been sent from Great Britain to the daily papers: Lord Lister has communicated to the Royal Society a paper by Dr. Allan Macfadyen, director of the Jenner Institute of Preventive Medicine, setting forth a prophylactic and curative treatment for typhoid fever. Dr. Macfadyen found that by crushing the microscopic cells of the typhoid bacillus in liquid air the cellular juices can be obtained apart from the living organism and that these juices are highly toxic. By injecting them in small, repeated doses into living animal its blood serum is rendered powerfully anti-toxic and bactericidal; that is to say, it becomes an antidote alike to living typhoid bacteria and to the poison that may be extracted therefrom. Dr. Macfadyen explains the application of the serum to animals and details his various experiments which showed that the serum is a curative of typhoid as well as a protective against infection. The Jenner Institute is now investigating the juices of other bacteria. If its experiments prove, as is expected, that bacterial juices in general react upon the animal organism in the same way as on the living bacteria which produce them, the fact will profoundly influence medical speculation and practice. Regarding the crushing of bacteria the question naturally arises, by what unimaginable accuracy of grinding can these infinitesimal organisms be broken so as to release their intercellular toxins. The crushing of the bacilli is done in liquid air because when thus frozen hard they become brittle and, notwithstanding their almost inconceivable minuteness, can be completely broken up by trituration and will under no subsequent conditions show a sign of bacterial growth.

UNIVERSITY AND EDUCATIONAL NEWS.

It is said that the suits over the will of Dr. Thomas W. Evans are now substantially settled, and the city of Philadelphia will receive a sum approximating $4,000,000 for the 'Thomas W. Evans Museum and Institute Society.' This institution is for 'the teaching of dentistry and for the display of his royal presents and personal effects.'

Mr. John D. Rockefeller has offered to duplicate money raised by Acadia College, in Wolfville, N. S., up to $100,000 before January 1, 1908.

Mrs. John Markoe, of Philadelphia, has given $5,000 to Harvard University to establish a scholarship in memory of her son James Markoe of the class of '89.

President Pritchett and other representatives of the Massachusetts Institute of Technology appeared before the committee on ways and means of the Massachusetts House of Representatives on March 11, in support of a bill designed to give the corporation of the institute power either to build over the whole or the part of the western two thirds of the block bounded by Boylston, Berkeley, Marlboro and Clarendon streets, or to sell it. President Pritchett indicated that the institute might be moved to new buildings on land owned by it near Jamaica Pond, Boston.

The physical laboratory of the University of Michigan will be extended this summer by the addition of a large lecture-room seating 400 and several other rooms for laboratory work. The top floor, which has hitherto been devoted to bacteriology, will be vacated by that department and will be added to the department of physics.

On Saturday, February 21, the University of Montana dedicated, with appropriate ceremonies, two new buildings, one a gymnasium and the other a woman's hall. The two buildings cost $40,000, but were not completed within the appropriation. An appropriation of $5,000 has been made by the legislature for their completion according to the original plans.

At Yale University Dr. Andrew D. White has been appointed Dodge lecturer on the responsibilities of citizenship, and Sir Frederick Pollock, of London, Storrs lecturer in the Law School.

Dr. George B. Halsted, late of the University of Texas, has been elected to the chair of mathematics at St. John's College, Annapolis, Md.

At Columbia University Albert P. Wills, A.B. (Tufts), Ph.D. (Clark), lately associate in applied mathematics and physics at Bryn Mawr College, has been appointed instructor in mechanics and mathematical physics; and Bergen Davis, A.M., Tyndall fellow of Columbia University, has been appointed tutor in physics.

Dr. J. E. Ives, instructor of physics in the University of Cincinnati, will leave his present position to go with the American de Forest Wireless Telegraph Company of New York City on the first of April next. To begin with Dr. Ives will have charge of the experiments with wireless telegraphy on moving trains and afterwards he will take up a series of investigations connected with the development of wireless telegraphy along commercial lines.

Dr. E. B. Brown, professor of Arabic at Cambridge University and fellow of Pembroke College, has been offered the mastership of the college in succession to the late Sir George Stokes.

Mr. Joseph Larmor, fellow of St. John's College, Cambridge University, has been elected Lucasian professor of mathematics in succession to the late Sir George Gabriel Stokes. Mr. Larmor is secretary of the Royal Society and is well known for his contributions to mathematics, his most important work being 'Ether and Matter.' The Lucasian professorship was founded in 1663 by Mr. Henry Lucas, who had been M.P. for the University. The first professor was Dr. Isaac Barrow, who resigned in 1669, Isaac Newton being elected to succeed him.